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Remarks on the Naturalization of Plants in colder Climates.

Addressed to the Editor by J. Mac Culloch, M.D., F.R.S., &c. &c.

In a paper which was inserted in your Journal, respecting the horticulture of tender plants, I had occasion to allude to the possibility, real or supposed, of rendering some, at least, of these more hardy, by raising them from seeds produced in a climate of a lower temperature than their original or natural one. At the same time I thought it right to state, that the opinions of the very learned and able secretary of the Horticultural Society were entirely opposed to this doctrine; opinions, I need not say, combining a weight of experience and authority which demand the greatest deference. I am uncertain whether he has himself published those opinions, but as the purpose of this slight communication is to pursue an amicable investigation, not a controversy, with the aid of those who may feel an interest in it, I am sure I cannot offend him by saying, that I have found those opinions, in a personal discussion of this point, at least as decided as I have here stated them. He will doubtless have the kindness to correct me in the same manner, if I am wrong; and will understand that the object of this publicity is the hope of exciting the attention, on whichever side, of those who have the desire and the means of bringing to the test of sufficient experiments what is a matter of pure experiment, and must finally be settled by experience. Like thousands more, I wish that the doctrine were true, or that it may ultimately be proved true; but, like all else in science, though too often overlooked in discussion, there can be as little pleasure as advantage in persuading others to receive error as truth, to adopt in this case as a fact that which, whatever value it may possess as one, would lead only to disappointment and evil, should it prove unfounded. And it is one of the most interesting questions in the philosophy of botany, whether in the relations of that science to agriculture or to horticulture, and to the endless modes in which these arts ramify and bear on human life. To assume such a power hypothetically, we see at a glance the enormous revolution it might produce in the present distribution of the vegetable world, and the extensive and valuable consequences that would result from it. To prove that the permanent alteration of the habits of plants, as to climate, is possible, even to a limited extent, would lay the foundation of many advantages, on which it would be as easy as it is unnecessary here to speculate. The extension and the greater economy of ornamental horticulture are obvious contingent results, of no small importance; and yet these would be of little moment, compared to the directly useful consequences in the naturalization of new products in agriculture, or, generally, in augmenting or varying the useful produce of the soil in a given country and climate.

Yet this question, practical as it is interesting, and a source even of entertainment, in whatever way it is finally decided, has scarcely received the slightest attention—none, it may almost be said, compared to its importance, whether as a question of botanical philosophy or utility, to its facility as an object of experiment, and to the amusement it is capable of affording. That also it must be a work of time, and may demand the continued trials of perhaps more than one generation of man, is an additional reason for thus urging it on the public attention, even where that which I have to offer directly on the total question is so extremely slender. But this paper is not meant as a contribution; it is intended merely as a stimulus to inquiry; and should it produce this effect, its end is answered.

On the general question of naturalizing the plants of warmer climates in colder ones, through the intervention of their seeds and through successive generations of those, I am fully aware that the fact was too generally assumed, at first, from two instances; and it may not be untrue that both of the most remarkable examples originally adduced as proofs were fallacious, or at least partially so. The one was the naturalization, real or supposed, of the Zizania aquatica by Sir Joseph Banks, and the other was that of the Canna indica in Guernsey, as noticed by myself. And, on my own part, I am quite willing to admit that the same error may pervade the other examples which I have noticed in the paper to which I have here referred in your Journal. It is unquestionable that, without reference to elevation, that great modifier of climate as constituted by latitude; without reference, in fact, to any external circumstances, there are, in any given climate, native indigenous genera, and species also, which are constitutionally more hardy than their immediate neighbours, and thus capable of bearing considerable changes of climate with impunity, where others, without any apparent reason that we can discover, fail. Such is the case, for example, with the very canna in question, compared to ginger or to the sensitive plant; while the cassia offers another familiar example of extreme susceptibility and repugnance to change, out of a crowd of plants from the same situations which we can cultivate without difficulty. The extent of the range of temperature occupied by the vine, by wheat, and by many more plants that I need not enumerate, shew that on this point there is great variety of susceptibility or hardiness in the vegetable creation; and a variety, of which advantages have been taken, and that may probably be far more widely extended hereafter. And this, in fact, forms one of the important parts of this very question, as it is one that has been neglected, a point to which I shall recur immediately.

In the meantime, to pursue the remarks on the two erroneous conclusions above noticed, it is as unreasonable as it is inconsistent with experimental philosophy, to reject the whole hypothesis, if it really be an hypothesis, on account of those. The vast extent of the subject demands a far wider range of trial; and if two, or twenty, such experiments have failed to establish its truth, neither are they sufficient to prove its falsehood. It is a question that demands, not simply more investigation, but investigation; nor will philosophy act justly in rejecting an even plausible hypothesis without more trials; it will be far, indeed, from right in ceasing from its investigation, if I can now shew, as I hope to do, some further reasons and some further facts, indicating that the opinion is not so utterly hypothetical as its opponents seem to esteem it.

As a *priori* ground of reasoning towards the recommendation of a sufficient course of experiments on this subject, the naturalization of tender plants, I may commence by pointing out the important fact just stated, namely, the great inequality of constitution in different plants which are natives of the same precise climate; of plants which, on a general or superficial view, we should reject or condemn to the house or stove. And this piece of knowledge is but recent, while, being recent, it is also limited as to the number of plants of which it has taken cognizance. There is no gardener now who does not know how many plants he has turned from the stove into the green-house; how many from the green-house into the garden or shrubbery, within these twenty, ten, five years; how many, indeed, are almost annually undergoing this migration, including plants of which, before those trials, he would not have believed in the hardiness. And yet the same fears or hesitation continue, while perhaps not a year passes in which many are not added to those which have undergone this change, a change no less agreeable to us than productive of unforeseen vigour and luxuriance in the plants themselves. I cannot, for one, forget that when first I knew Scotland, there was not a plant but the most rude and hardy attempted to be cultivated, or even placed for a summer, in the open ground; that I first pointed out myself those tenderer plants which I believed capable of enduring that climate out of doors, and that I now see everywhere flourishing in the borders the most luxuriant plants of heliotrope, fuchsia, verbena triphylla, geraniums, and numerous others, replacing the wretched, starved specimens formerly nursed with the greatest anxiety in greenhouses.

Here, then, is the first ground of trial. We do not yet know what plants out of the hotter climates of the whole world will bear our climate, and it is certain that without trial we never shall know. Nor do we know in what their peculiar delicacy of constitution consists; what are their antipathies; what the causes of their repugnance and suffering. They are living beings, and in a sense they are sensitive ones; since the effect of poisons proves that they possess something in the nature of a nervous system, even did not that admit of ample proof from other phenomena, as I have fully shewn in an essay on this particular subject. We have assumed that mere difference of average temperature, or of extreme temperature, a heat too low, is the sole cause of their death and our failure. This is a hasty and a superficial conclusion; it may be true, but it is not proved, and there are various scattered phenomena too minute to be enumerated and insisted on in a sketch of this nature, which may render it doubtful. Soil is not an indifferent question, nor is the quality of the atmosphere, hygrometrically considered, nor the light, the winds, the situation, the exposure, the shelter or otherwise, nor the particular mode in which water is applied, or in which wet and dryness alternate, nor more, on the details of which I need not enter, as they will suggest themselves to every scientific horticulturist.

Hence, then, it may be now actually in our power to adopt plants which we have hitherto feared or rejected without trial; but it is further possible that the process in question, viz., that of sowing native seeds, which has failed to naturalize some, may not fail in all. No one can decide against this beforehand, because the trials have not been made. Admitted that the caper, or still more the pine-apple, might be beyond hope, it does not follow that every plant from the same climate should be, or that we are unable by cultivation to effect a slight change, because we cannot make a great one. I myself believe that the canna, though admitted to be a hardy plant considering its climate, is now a hardier one from the seeds raised in Guernsey, than from those imported from the West Indies; but it is so difficult to be absolutely sure of any experiments of this nature, except under a long course of trial, that I do not claim the assent of others. I can only repeat, let the experiments be fairly and amply made, because nothing else can determine the question, and let not that be rejected which offers even a probability or a hope.

To proceed with other *priori* reasoning on this subject, demanding reflection at least, if not actually proving anything.

The preceding discussion rests solely on the hypothesis, or fact—that the method, and the only one, of naturalizing a plant to a colder climate, consists in saving the seeds produced in that climate, and in again sowing the produce of that produce, and thus in succession. That original proposition has also been always connected with this other,—namely, that no plant produced by slips or cuttings ever became hardier than the parent, through whatever succession of progeny thus formed. Is this very certain? I much suspect that, if attentive gardeners will be at the trouble of reviewing their experience carefully, they will find that it is doubtful. I believe it is not true; though it would be useless for any single individual to state the specific cases on which he founds his opinion, so delicate are solitary conclusions like this, and so little entitled to command assent. But a familiar fact, if less noticed even by planters than it demanded, will perhaps render that probable which cannot here be proved.

This consists in the different degrees of hardiness displayed by the same plant in different situations, or under different degrees of injury from climate, chiefly, perhaps, from winds, but apparently from differences of temperature also. A familiar example is seen daily in woods,—most easily, perhaps, in fir woods. Here, it is notorious, that the interior trees become feeble or delicate, putting out of question the important fact of the loss of their branches and leaves from want of light, while those which are most exposed produce strong lateral ramifications and also stronger roots; or, generally, that the more a tree is exposed to the injuries of climate, within certain limits as to its powers of resistance, the more sturdy and vigorous it becomes. But this is not all. If the feeble and over-nursed tree be now exposed, provided that exposure also be regulated to its powers, it gradually assumes the strength which it would have possessed if originally exposed, or tends, with all its powers, to approximate to that condition.

This, then, is a vital or sentient principle;—the peculiar sentient principle of vegetables, whatever that be, exerting itself to repel injury; or, without speculating on the cause, such a plant is, by its own efforts, however excited and exerted, attempting to naturalize itself to a new situation or a worse climate. It is attempting to become hardier, and it does become hardier. Is it then so certain that our foreign plants, propagated from slips, do not become hardier on the same principle, and is it certain that they have not become so? We assuredly have not decided on the negative; and it is more than disputable; though at the same time we must take care not to overrate this power, should we admit it; since, even in our native trees, under the before-mentioned changes, we see that it is limited.

Again, to pursue this species of reasoning, and to extend it, as far as it is extensible, to the case of seeds, and of successions of seeds. In spite of some recent assertions from France, that bad seed-corn, in the popular estimation, was equal to what was usually thought the best, or even that it was better, I believe I do not misstate the general, the perhaps almost universal opinion, that in plants generally, the most vigorous produced the best seed, and also that such seed gave the best chance of plants equal to the parent. Even should this be not fact, the very familiar one, that to obtain good varieties of a cultivated tree we must sow seeds from a good variety (that no one would sow the seeds of crab-apples, for example, with the same hopes or results as those of golden pippins), serves to prove that the vigour or character of the plant does influence the seed, and, through that, its progeny. There is a new character, of some kind, acquired by cultivation, somewhere between the soil and the seed, we know not how or where, and that character is propagated to the succeeding plant, or to some one at least of the whole offspring; or else there is a character propagated which did not belong to the original plant of all.

Here, then, is a propagation to progeny, of characters acquired by the vital actions of a plant under cultivation, or under changes which man induces, often not knowing how, in a natural plant. And these characters are no less various than singular. What they are in general, is familiar to every one; for such is the basis and essence of all our economical horticulture, and of much at least of our agriculture, if not of all. Hence our fruits, our seeds, our esculent vegetables in all their varieties, and hence our flowers.

On these characters I need not dwell; they are known to every one who knows anything of this subject. The ostensible ones at least are familiar; bulk, colour, flavour, and so forth. Is it certain that superior hardiness is not sometimes one of these? This is the point that interests especially the question before us, and it almost answers itself. At least there is not a gardener who cannot answer it with respect to some flower and some fruit, to many flowers and to many fruits. Examples would be as superfluous as endless. One case (and it may be real or assumed) will answer the present purpose as well as a thousand. The peach is naturally one. It has produced and produces peach-trees which ripen their fruit more or less easily, or more or less early, in different climates or temperatures: or there is a hardy peach-tree, the produce of one less hardy, and capable of propagating other hardy peach-trees. Among other characters, therefore, which may be acquired by cultivation, there may be acquired that of bearing a colder climate, and of propagating, occasionally, if not in every instance, a progeny as hardy, or even more hardy. I presume I need not say that to bear fruit early, instead of late, is hardiness; as it is to bear fruit at all in certain climates, in Scotland, for example, compared to England; while I also need not say that any gardener can furnish the requisite facts.

And this character is propagated through the seeds; though we may not be able to define in what part of the plant it is formed, nor how it is generated. Nor is it a deadly objection that it is not universal nor permanent: it is sufficient if the fact has been shewn to occur, and it remains with us to apply it and regulate it more widely. The change of progeny through seeds is, therefore, not impossible; for it happens, and it happens in the very mode which the hypothesis assumes, if not to the entire extent. A hardy progeny is obtained out of the seeds of a plant less hardy: a basis at least for our trials as to the particular object in question is established, though the entire hypothesis remains unproved: to pursue these, and, finally, to prove it, or otherwise, is, once more, the question at issue.

And it is at issue already on somewhat more than bare *priori* reasoning; though I commenced with that mode of argument only, since the remarks just made are facts in point. It remains, therefore, now, to see whether any further ones can be added to those formerly adduced, and those now given in support of the argument; facts to shew that it is true to a more decided extent than may be derived from the preceding remarks on the varieties of plants produced by cultivation, and facts perhaps less easily questioned than those which were formerly brought forward, and which I really feel no great inclination to defend strongly.

In that paper, I believe, in a work on the subject of wine, at least, I took occasion to hold out this theory of naturalization through seeds, as a probable mode of cultivating the vine in this country more successfully than has yet been done. Or, speaking more decidedly than the facts actually warrant, for the sake of simplicity, it was suggested that we might in this manner produce varieties of the vine which should ripen their fruit out of doors, under no further casualties at least than occur in the ordinary climates of this plant. I was unwilling then to state a fact on this subject which had been reported to me, because it was not confirmed by parallel trials; that confirmation has occurred, and I need not, therefore, longer suppress it, while I have no reason to doubt the correctness of the report.

This is the case of a seedling vine growing at Hackney, the produce, as I am informed, of the black Hamburg grape, and remarkable, during a long course of years, for the early and unfailing maturity, as well as the abundance and goodness of the produce. For the confirmation and consequent extension of the principle as well as the fact, I am indebted to M. Lenoir, a recent French writer on the cultivation of the vine, and the art of making wine. And although this author does not attempt to draw the general conclusions which favour the main object of this paper, he is fully sensible of the value of the fact as it relates to the cultivation of the grape for wine, and does not fail to lay stress upon it.

The first example quoted is of a seedling vine raised by M. Van Mons, at Brussels; the result being a fruit as large as a greengage plum, which, at the latest, ripens in the first fortnight of August, and which never fails. Its qualities are no less remarkable, as it is both solid and sweet. Now this instance is important, because the vine cannot be cultivated at Brussels in the open air to any purpose. The only grapes ever produced there are from vines sheltered in walled gardens, and trained on espaliers or otherwise; nor, even thus, is the success much, since the fruit is always very late, and seldom ripens completely. The author, who has in view a different object from mine, remarks on what might be hoped for from the extension of this practice in a better climate, when the results are such in one so unfavourable. And from the general tenor of his observations, we are taught, that even in the wine countries of France, where the cultivation of the vine is no less an object of care and study than the manufacture of wine, the practice of raising varieties from seed is scarcely known. Hence it is that not from the works of the French agriculturists, and not even on this single point, are we able to discover sufficient facts to aid much in forming our opinion on the general question. I will, therefore, pass over another reported by Rozier, since the only deduction made from it relates to the produce in wine, not to the quality, in any shape, of the new plant. That this professed philosopher in vines and their treatment should have overlooked the important consequences which might have been deduced from the study of a whole vineyard of seedlings, is a very natural object of surprise and censure to M. Lenoir; and it is one that we may fairly echo as to our own horticulturists, on the whole of this interesting question.

The next, and only other decided fact which I shall here quote, proving that a hardy variety of the vine can be produced from seed, will be found in Duhamel; and he also has entirely overlooked those important, and at least possible, consequences which form the object of this notice: it is that of a vine at Chaillot, near Paris, in the garden of the Chevalier Jansens, which ripened completely every year, and produced excellent fruit, while the plant itself was an example of uncommon strength and luxuriance. The seeds were taken from a variety called the Verjus, to which I cannot give a corresponding English horticultural name, if indeed there be one. But that which alone is of much importance in the matter is, that the variety in question scarcely ever ripens at Paris; the very best result, and that only in the hottest seasons, being a faintly sweet, almost insipid produce.

Here then are two instances, resting on the best authority, of hardy or hardier varieties of one plant at least, and that a most important one, produced from seed; while I hope that the example at Hackney, for which, however, I do not presume myself to vouch, will be found a third. And how often has the experiment of seedling vines been tried? Did we judge from the general tenor of Lenoir's remarks, perhaps not a hundred times throughout all France. English gardeners will know better than I do what the trials in this country have been; but let us at least be furnished with a large list of failures before we decide that two or three instances of success do not afford ground enough for continuing these trials. And be it remembered also, that three successful examples are the results but of one succession, a single generation. If the original hypothesis is not absolutely visionary, the experiments will not be decisive, except under many more generations; and surely at least, be it the merest of hypotheses, the subject deserves that much of investigation.

I need not proceed, however much illustration I might yet add; as my object is not to discuss this question, but to excite to examination; and I am desirous that this communication be as brief as possible. But it may be right first to state how the hypothesis ought perhaps to be modified, that it may not be loaded with greater difficulties than it merits, or lead to greater disappointments than it is willing to be blamed for.

Abandoning the too broad principle, that any tender plant may be rendered more hardy through native seeds, and that successions of such seeds will carry this acquired constitution still further, let us commence with remembering that the sowing of the seeds of cultivated plants does produce new varieties, and that such varieties also produce others in succession, by continuing the processes of cultivation and sowing. Next, it is certain that there is a variety of character and constitution in these varieties; and here it is admitted, that, instead of gaining an improved progeny, we sometimes find a worse one, a crab, for example, from the seed of a pippin. This then may be a source of failure in single instances; but it stands in the nature of exception, and does not justify the abandoning of the experiments. If it did, he who had obtained crabs from pippins would abandon sowing; we should have, perhaps, possessed no horticultural produce of any kind. Let the produce, therefore, be watched; and if we now watch for the sake of selecting the best fruits, let us see also if we shall not discover a plant of a more hardy constitution than its parent.

Such a plant there may be, one at least out of many seeds; and in truth if this were not the case, we could not have had one fruit of any specific nature more hardy or precocious than another; or, in the examples above quoted, the vines of M. Von Mons or Jansen. This is the plant, should it occur at all, whose seeds would require our especial attention; just as he who desires to produce new apples chooses his seed from good fruit.

Thus, then, let the hypothesis be modified, and then the experiments made, before we agree to reject the one and neglect the other. But this is not all. We are ignorant of the constitutions of specific plants, of their antipathies, of their sensitive systems, as I have shewn; at least our knowledge is extremely narrow. We have found some hardy, which, and for apparent good reasons, we had imagined tender. It may be that these very plants, or some of them—it may be that others (such is our ignorance)—may afford facility of naturalization through sowing, though others do not. Let us try wherever we can; for it is certain that we are unable to pronounce on negatives, and that, here, probabilities at least present themselves. Whatever we gain will at least be an acquisition; and the whole range is so extensive, that did we succeed but in one instance of a hundred, our advantages would not be small, to whatever department of horticulture or agriculture we may turn our eyes. May I hope that my learned friend, the Secretary, will join me in encouraging investigations opposed to his own opinions? but indeed I cannot doubt that he would be among the first to rejoice that they should prove unfounded, since, with all philosophers, his aims are utility and truth. Most gladly shall I see from his pen, facts, be they what they may, which my own situation prevents me from pursuing, and ever will.

I need scarcely add more than a few lines on the expected practical advantages, since I could do little more than repeat what I have said on former occasions; and yet a few will not be useless, for the sake of those who, without them, might consider the whole discussion as a mere idle question of curiosity. To command flowers in greater variety and beauty, in greater luxuriance and profusion; to render the more rare as common as the more vulgar, and at a less expense, is a worthy object of horticultural science. To augment the variety and beauty of our shrubberies is a worthy object; and he who compares a shrubbery of 1700, of almost a century later I might say, with the shrubbery of the present day, will not forget that all this has arisen from exertions even perhaps greater in their way than what are here pointed out. Have we not also neglected our woods and plantations? Have we, at the same time, naturalized and rendered common ten, nay five, forest trees since the time of the Romans? We surely cannot have arrived at our limits, either in power or utility, among the almost innumerable trees of the world. Of the vine, in another department, I have already spoken; and here, surely, not only is the temptation great, but there is a prospect of success, for the fact is ascertained. No rational economist will desire to see the vine become an object of actual rural economy; but passing by this, much pleasure, and even profit, might be derived from such improvement in that plant as would admit of its use in the manufacture of native wines, did they but replace the endless nauseous compounds under this name, made from fruits that never furnished *wine*, and never will. The potato also, that I may end these slight remarks, is still a tender plant; a melancholy fact, far too well known in Scotland, however little English cultivators may be aware of it. If we have produced some hardier varieties of certain fruits by care and cultivation; if we have a juneating apple as well as a russet, a summer as a winter pear, who shall say that an assiduous cultivation of the potato may not hereafter produce a variety that will ripen its roots in August instead of October—ripen them before a Scottish winter arrives to destroy the only dependence of the starving occupants of five rocky Highland acres.

But I must end; and I would apologize to you for again intruding this subject on your Journal, did you not know, as well as myself, that in the present rapid succession of inquiries and opinions, that which is once out of sight is immediately forgotten, and that he who desires to call the attention of the public to any object must attempt it by perseverance and repetition. Thus have I done on the question of naturalizing sea-fish, if as yet to little purpose; while desiring nothing but investigation, I am ready to abandon both subjects as soon as any one will take up the pen in my room.

I am, dear Sir, yours, &c. &c.

A Medico-Chirurgical Commentary on Shakspeare.

By William Wadd, Esq., F.L.S.

Shakspeare has been allowed, by every commentator and critic, from Rowe to Douce, the meed of a matchless knowledge of human nature; but, while all agree in giving credit to the unrivalled power of his genius, some have questioned his learning and attainments.

Yielding to no one in my admiration of our great dramatic bard, and fully sensible of the services these interpreters have rendered him, I would crave leave to notice one point, which seems to have escaped their observation, or, if they have alluded to it in the abstract, I do not recollect any practical illustration;—I mean, his decided knowledge of abstruse sciences. Let us take that of medicine, in all its branches, as an exemplification of this remark. One can easily understand how an intuitive knowledge of mankind might enable a genius like his "to hold the mirror up to Nature," and even to kindle into life new beings and new worlds:—

> Each trait of many-coloured life he drew,
> Exhausted worlds, and then imagined new.

Or further, that his nice discrimination might render him a faithful delineator of human passions, and human actions, but that he should be versed in all "the thousand ills that flesh is heir to," and understand the proper treatment of those ills, and the niceties of medical and surgical practice, with which he exhibits in his writings so intimate an acquaintance,—this is beyond what professional faith can allow to intuitive genius alone. If he did not, indeed, study medicine and surgery, we must agree, with Pope, that "he gives ground for a new opinion, that the philosopher, and even the man of the world, may be *born* such, as well as the poet."

The medical profession has undoubtedly furnished some geniuses of the first order; but it would be a grand acquisition to identify a doctor in our immortal bard! We should be proud to hail him as a surgeon; the Apothecaries' Company, and the general practitioners, would probably be equally anxious to prefer their claims.

The history of Shakspeare's life, however, does not give us any reason to believe that he was otherwise connected with the medical profession, than by having a medical son-in-law (Hall.) Whether he profited by this connexion or not, we cannot tell; but we have as little ground to imagine that Hall was of use to Shakspeare by his medical talents, as we have that Shakspeare assisted Hall; and yet, the intimate knowledge displayed by "the bard of Avon" in physic and surgery, pharmacy and physiology, leads to the conclusion, that he must have acquired his medical notions from practical experience.

With singular felicity and discernment, he hits upon the particular point on which a medical or surgical argument would turn:—it does not appear that he ever *walked the hospitals;* yet I can hardly conceive that the ordinary *walks of life* would furnish means for describing symptoms, as he does, with the technicality of Warren. He might challenge Sir Henry Halford, to define with greater accuracy the type of febrile exacerbations; and so nice is his discrimination, that, with the correctness of a clinical lecturer, he notices an intermittent fever passing into a continued form, and makes a "quotidian tertian," and "delirium tremens," end the life of Sir John Falstaff, whose nose (as in such cases) became "sharp as a pen." (*Hen. V.* act ii. sc. i.)

With hypercritical acumen, he makes a distinction between *distemper*, and *disease.* In using the latter word, it is always applied to malady, or deadly sickness.

> What's the *disease* he means?
> 'Tis called the evil.

Again,—

——— We're all *diseased,*
And with our surfeiting and wanton hours
Have brought ourselves into a *burning fever.*

When, on the other hand, he uses the former word, it is lightly touched, as,

Little faults proceeding on *distemper.—Hen. V.*

And in *Henry IV.*

'Tis but a body yet *distempered.*

Concerning hysterics, there is another practical refinement in *Lear,* act ii.—"Oh, how this *Mother,*" &c. by which application of the term he evinces a belief, that this affection was not peculiar to women, but common also to men; a doctrine entertained by the best medical authorities of that period.

What modern physiologist could give a more correct representation of the relative conditions of the organs of circulation than he does, where he describes the phenomena which they present in the gasps of mortal agony, when the blood, for the last time, has retreated to its citadel?

Oft have I seen a timely parted ghost,
Of ashy semblance, meagre, pale, and bloodless,
Being *all descended to the labouring heart,*
Who, *in the conflict that it holds with death,*
Attracts the same for aidance 'gainst the enemy;
Which, with the heart, there cools, and ne'er returns
To blush and beautify the cheek again.

Sympathies he understood as well as Kenelm Digby; and how well he comprehended the influence of the mind upon the body, is demonstrated by that masterly scene in which Henry VIII. hands over some papers, of no very agreeable import, to Cardinal Wolsey:—"Read this,—and this,—and after, this,—and then, to breakfast with what appetite you may." Abernethy could not have put it more pithily! Here was a touch upon the gastric nerves, with whose effect on digestion he was well acquainted, as also that produced upon the faculties by gross feeding; he knew how uncongenial *gastronomic* and literary pursuits must ever be:—

Fat paunches make lean pates, and grosser bits
Make rich the ribs, but bankerout the wits.

And, for this reason, he invariably abstained from composition when he retired to aldermanic fare at Stratford.

Then again, how ably and beautifully does he delineate the effects of suppressed mental emotions :—

> She never told her love,
> But let concealment, like a worm i' th' bud,
> Feed on her damask cheek.

And, in the same tone, in another place :—

> To keep it from divulging, let it feed
> E'en on the pith of life.

Nor was he ignorant of idiosyncrasies, or constitutional peculiarities :—

> Some men there are, love not a gaping pig ;
> Some that are mad, if they behold a cat—
> ——— for affection,
> Master of passion, sways it to the mood
> Of what it likes, or loathes.

When he asks, whether the doctor can

> Minister to a mind diseased,

and evinces, in mental aberrations, his want of faith in physic, he only acts according to the opinion of his contemporaries—it is no proof of ignorance, for in other places he shews an intimate acquaintance with many of the leading features of the distempered mind, particularly the increase of muscular strength :—

> My limbs,
> Enraged with grief,
> Are thrice themselves.—*Henry IV.* Part 2. act i.

His tact in symptomatology, is evinced in the following description of the febrile paroxysm of an intermittent :—

> He had a fever when he was in Spain,
> And when the fit was on him, I did mark
> How he did shake: 'tis true, this god did shake;
> His coward lips did from their colour fly,
> And that same eye whose bend doth awe the world,
> Did lose its lustre. I did hear him groan;
> Ay, and that tongue of his, that bade the Romans
> Mark him, and write his speeches in their books,
> Alas ! it cried, Give me some drink, Titinius,
> As a sick girl.—*Julius Cæsar,* act i. scene ii.

Now, let us look to his surgical pretensions, and we shall find strong symptoms indicative of a minute knowledge of anatomy, and surgery. Take, for instance, his repeated reference to the *pia-mater*[*], and his notice of the *epidedimis.* How honestly does he admit the impossibility of curing scrophula, when he describes those affected by it, as

——— Strangely visited people,
All swoln and ulcerous, pitiful to the eye,
The mere despair of surgery!—Macbeth.

And, from the manner in which he sneers at the rubbers and thumbers, we may conclude, he would not have been a subscriber to modern quackery[†]. His contempt for charlatanerie seems proportional to his scientific knowledge of surgery; indeed, he was fully informed of the use of the lancet and cautery: "We do lance diseases in our bodies[‡]," says he; and in *Timon of Athens*, act v. scene iii., actually speaks of "cauterizing the root of the tongue;" and cicatrice, a technical term, is of frequent occurrence. Podagram and chiragram, he also speaks of, and what has jocosely been designated *hip*-agram, he correctly calls *sciatica*—(*Measure for Measure*); and who, out of an hospital, ever heard such a list of surgical diseases, as Thersites runs over to Patroclus? (*Troil. and Cressida.*)

In numerous instances, he seems giving a prognosis. "This apoplexy will certainly be his end!" (*Hen. IV.* act i. scene ii.) Sir Astley Cooper could not be more positive, nor could he describe it more appropriately than by the words in *Coriolanus*, "A very apoplexy,—lethargy, mulled, deaf, sleepy, insensible;" and there is no doubt that when, in *Othello*, he states, that "My Lord hath fallen into an *epilepsy*," (act iv. scene i.) he could, had it been necessary, have defined that extraordinary disease with equal accuracy; for even such as result from sedentary occupations did not escape him, as he unequivocally states, that "universal plodding, prisons up the nimble spirits in the *arteries**." (*Love's Labour Lost,* act iv. scene iii.) Or had he studied the chylepoietic doctrines of our times, could he have given a more decided touch of practical experience, than the following remark in *Coriolanus?* "Your affections are a sick man's appetite, which most desires what would increase the evil."

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[*] *Pia mater*, Troil. and Cressid., act ii. scene i., and Twelfth Night, act i. scene v.

[†] We may not be so credulous of cure,
When our most learned doctors leave us; and
The congregated college have concluded,
That labouring art can never ransom nature
From her unaidable estate. We must not
So stain our judgment, or corrupt our hope,
To prostitute our past-cure malady
To empirics.—*All's Well that ends Well*, act ii.

[‡] Ant. and Cleop. act ii. scene i.

His knowledge of the liver is evinced in many instances; and in *Love's Labour Lost* he speaks of the "liver vein," in allusion to the ancient notion of its being the seat of love. The "red plague," which Caliban so kindly wishes may seize upon Prospero, was another term for erysipelas. And when he speaks of serpigo, he distinguishes the "*dry* serpigo"—"Now, the dry serpigo on the subject†." From these critical niceties, his remarks on stewed prunes‡, and some hints about the tub-fast, it is clear that he had studied Master William Clowes's Surgery. And, although he does not use the modern term, "morbid poison," perhaps thinking *all* poisons *morbid,* or some other reason, yet, from his joke about a French-crown §, his equivoque on the corona veneris, and the manner in which he alludes to the devouring powers of the "goujeers," it is evident that he understood the actions of animal, as well as vegetable and mineral, poisons. He would have split a straw-argument with John Hunter or Dr. Adams, on yaws or sivvens, discussed the *pseudo* with Abernethy, and defined phagedena with the accuracy of Celsus. Of the sweating-sickness, and the proposed remedies, he was perfectly master: there is abundant proof that he was equally versed in the symptoms of leprosy; and it is more than probable that he knew the difference between the Cochin leg, and the elephantiasis of Aretæus.

How minutely he was acquainted with the *paraphernalia* of surgery, is curiously exhibited in the quibble on the word "*tent:*"—bring in the "surgeon's box, or the patient's wound;"* and he gives to Dr. Caius a "boitier verd," which is a truly technical term, for a case of surgeon's instruments.

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* The system of physic of those days gave to the *arteries* the office now given to the nerves, as the derivation of the word implies, ἄερα τηρεῖν.

† Troil. and Cressid., act ii. scene iii.

‡ First Part of Hen. IV.

§ Measure for Measure.

Had he served seven years' apprenticeship to an apothecary, his acquaintance with the properties of chemicals and galenicals could not have been more correct.

He evidently well knew the effects of belladonna; the "insane root," he tells us, "takes the reason prisoner;" and he was equally master of the properties, and effects, of sedatives, for he explains a case where "poppy nor mandragora"† would avail not, and, in mixing a dose for Othello, he makes it "as bitter as coloquintida." (Act i. scene iii.)

Many curious and rare substances in the vegetable world are critically treated.

The nettle of India, the urtica marina, a zoophite in the Indian sea, is even noticed.

Yon dwarf, of *hind'ring knot-grass made,*‡

shews a knowledge of ancient notions; this herb being supposed to have the power of stopping growth, and fern-seed that of making people invisible, as well as curing the rickets. Then, the "cuckoo-buds of yellow hue,"§ is a remark of one skilled in botany. Gerard calls it, "Flos cuculi cardamine."

In *All's Well that ends Well,* he makes Parolles notice Galen and Paracelsus, in a manner that clearly proves he understood their different systems. He applies the term *learned* to Galen, and *authentic,* or *fashionable,* to Paracelsus; and when he speaks of all the learned and authentic fellows, he uses the very words of the DIPLOMA, "*authenticè* licentiatus."

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[*] Troilus and Cressida, act v. scene i.

[†] Other properties of mandragora are alluded to by him, besides the narcotic. Gerard, in his Herbal, says of the mandragoras,—"Dioscorides doth particularly set downe many faculties hereof, of which, notwithstanding, there be none proper unto it save those that depend upon the drowsie and sleeping power thereof."

In Adlington's Apuleius, (of which the epistle is dated 1566,) reprinted 1639, 4to. bl. let. p. 187, lib. 10:—"I gave him no poyson, but a doling drink of *mandragoras,* which is of such force, that it will cause any man to sleepe as though he were dead."

[‡] That verbose gentleman, Mr. Apothecary Tomlinson, 1657, informs us, that "*knot-grass is a low repentent herb, with exile, copious, nodose, and geniculated branches.*"

[§] Love's Labour Lost, act v.

He may be said to have been "bon GALENISTE, et très bon PARACELSISTE." He seems to have considered the doctrine of Galen as "non mesprisable pour la pathologie, et *profitable* pour les boutiques." While that of Paracelsus was "bonne à suivre pour la subtilité, pour l'épargne, et pour la therapeutique."

From the familiar manner in which he alludes to certain pharmaceutical operations, it is evident that he did not consider the apothecary the mere "culler of simples;" and when he states the courses quicksilver takes, (*Hamlet*, act i. scene v.,) he proves himself acquainted with all the subtleties of its application, for though out of fashion now, the time has been, when it was as common to take a drachm of quicksilver with a dish of coffee, as it is now to take a "chasse caffé" in the shape of a dram of mareschino.

Then his observation about—

> —— The toad, ugly and venomous;

and the

> Precious jewel in his head,

shews that he was acquainted with the notion of the early naturalists, that the head of an old toad contained a wonder-working medical-stone; and although a modern apothecary would not look there, for a "most soveraigne remedy to repulse poysons," yet, Master Edward Fenton, 1569, tells us that "there is founde in the *heades* of olde and great *toades*, a *stone*, called borax or stelon." (*As You Like It*, act ii.)

That he was skilled in comparative anatomy could be easily proved; and that he was abundantly stored with materials of natural history is equally true; even the building of the swallow's nest in a favourable aspect did not escape his observation.

That he was also deeply read in Tuberville's book of Hunting, and the noble art it treats of, cannot be disputed; and from the manner in which he alludes to the "staggers," as "the horse's apoplexy," and other diseases, it may fairly be inferred, that he was not ignorant of the veterinary art.

After all this evidence in proof of the medical attainments of Shakspeare, we are at a loss to reconcile such an unprofessional speech as—

> —— Throw physic to the dogs;

and that still more injurious one,

——— Trust not the physician,
He kills more than you rob.

It must be confessed that, having uttered such opinions, he could not, however qualified, have had the assurance to take out his M.D. diploma. As, however, in no one instance can he be accused of giving offence to *surgery*, we may be allowed to indulge a hope, that had he not been contented with being one of the most renowned, of that bright galaxy of genius who paid their court to the

——— fair Vestal crowned in the west,

with being, in short, the pride of England, to all succeeding ages, it was in his power to have been—as Hunter now is—the glory of the *Royal College of Surgeons!* *

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* Since the above was written, and indeed printed, we find Sir H. Halford calling the attention of the College of Physicians to the very same subject.—"Sir Henry observed, that in the closet scene in Hamlet, the following words occur:—

'————— Extacy!
My pulse, as yours, doth temperately keep time,
And make as healthful music; 'tis not madness
That I have uttered; bring me to the test,
And I the matter will reword—which madness
Would gambol from.'

"The circumstance to which the learned President particularly alluded, was the expression 'I the matter will *reword;*' and he proceeded to relate the following case, in illustration of the justness of Shakspeare's 'test.' He was called, last January, to a gentleman, then in a state of mental derangement. A short time previous to his illness, he had sent for his solicitor, and given directions about his will. He stated his intention of adding 500*l.* a year to his mother's jointure, and of leaving various legacies; adding that his friend, the solicitor, was to be residuary legatee. The solicitor, in the most honourable manner, told him that he could not consent to the last part of the arrangement unless at the end of six months he continued of the same mind upon the subject. In the interval, he was attacked with mental excitement, for which he was attended by Sir Henry Halford and Sir G. Tuthill. One day, on asking him how he did, he appeared calm and collected, and answered that he was very ill, and only anxious to settle his affairs and make his will. Next day he repeated the same expressions, in a tone and manner which induced his attendants to comply with his request, and the solicitor was sent for, who brought with him a will drawn up according to the instructions he had formerly received. This was read over to the gentleman, and being asked, after each clause, if such was his meaning, he distinctly replied—yes, yes. The will was then executed, being witnessed by his physicians. On going down

Elements of Medical Statistics: containing the Substance of the Gulstonian Lectures delivered at the Royal College of Physicians; with numerous Additions, illustrative of the comparative Salubrity, Longevity, Mortality, and Prevalence of Diseases in the principal Countries and Cities of the civilized World. By F. Bisset Hawkins, M.D., &c. London, Longman & Co. 1829. Pp. 234.

This is really a very interesting work, for which we think the public, and the medical profession in particular, stand deeply indebted to Dr. Bisset Hawkins. It is quite obvious, from the whole tenor of the book, that the subject is a favourite one with the author. He writes upon it *con amore*, and enters into all its details with the precision and fulness of an enthusiast. It will not be going too far to characterize the work as the first of its kind, not only in this country but on the continent. The author, indeed, takes occasion, in his preface, to allude to this circumstance, and to extract from it an apology for any inaccuracies or omissions which may be detected. These are clearly inseparable from a first attempt to put together facts and observations which have no very close or obvious connection with each other; but, though we shall not shut our eyes to the imperfections of the volume, we rather marvel at the skill by which the author has contrived to link together such heterogeneous materials. He has evidently been long engaged in the task. There are no marks of hurry in the composition of the book; every portion of the subject has received its due share of attention.

It is properly a book of reference, small in bulk, but rich in material. Dry details constitute its principal value; and though some pleasing reflections are dispersed here and there through its pages, couched in very elegant language, still these are few in number, and can hardly be viewed as an essential feature of the work. We shall find some difficulty, therefore, in giving an adequate sketch of the materials of which this

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stairs, Sir Henry observed upon the unpleasant circumstance of the medical attendants becoming involved in a deed which was likely to become the subject of litigation, and proposed that they should return to him, and apply Hamlet's test, by ascertaining whether he could 'reword' his will. With regard to several of the clauses this was the case; but he stated that he had left one individual ten thousand pounds, whereas he had only left him five thousand; and on being asked to whom the residue of his fortune was to go, he answered, 'To the heir at law to be sure!' Being asked who was the heir at law, he replied that he did not know. 'Thus,' said Sir Henry, he could not 'reword' his meaning, but 'gambolled' from the matter.'"

volume is composed. Condensation being impossible from the very nature of the subject, *selection* is our only alternative. We shall bring before the reader those points which appear to us to be of most general interest; and from the mode in which Dr. Hawkins has elucidated them, he will form for himself an estimate of the manner in which other topics are discussed, which, possessing less interest to the general reader, are perhaps of superior importance in the eyes of the physician.

Medical Statistics is, at best, a very awkward term; nor is it easy to define accurately its meaning. Dr. Hawkins characterizes it as the science which illustrates the natural history of man, in health and disease, by the application of numbers. It has for its principal objects to determine the rate of mortality in the different regions of the world; to ascertain the comparative salubrity of different countries, and the kind and extent of disease prevalent in different districts, in different sexes, in the different ranks of life, in different trades and occupations, at different ages, and under different circumstances of political existence. The term *Statistics* appears to have been first used about the middle of the last century by a German Professor of the name of Achenwal, and applied by him to express a summary view of the physical, moral, and political condition of states. Some notion of statistical inquiries existed in the time of the Romans. According to Ulpian, (a lawyer, who afterwards became the secretary and principal minister of Alexander Severus,) registers of population, puberty, age, sex, disease, and death, were kept with exactness by the censors from the time of Servius Tullius to Justinian, comprising a period of ten consecutive centuries. But as these registers embrace merely the citizens of Rome, and not the slaves, the results apply only to select or *picked* lives. It appears from these and other documents, that the mean term of Roman life was thirty years,—that is to say, taking 1000 persons, adding together the years they individually attained, and dividing the total by the number of persons, the result is thirty. It is curious to contemplate the improvements which have been effected in the course of time in the value of human life. Mr. Finlayson has ascertained, that in England, at the present time, the expectation of life, for persons similarly circumstanced, is at least fifty years, giving us a superiority of twenty years above the Roman citizen. The mean term of life among the *easy* classes at Paris is at present forty-two. At Florence, to the *whole* population, it is still not more than thirty.

Dr. Hawkins first treats of the mortality in Great Britain, which he afterwards compares with that of other countries.

The results are highly satisfactory; and we think that our readers will be gratified by knowing how well off they are at home, and with what little reason our continental neighbours shudder at the fogs of England, and boast of their own brighter skies and more genial atmosphere. No one point in the volume before us is more assiduously laboured, nor more clearly made out than this; and a large debt of gratitude is certainly owing to Dr. Hawkins by every Englishman, (especially if he be a traveller,) for thus "binding him to his native mountains more." In 1780, the annual mortality of England and Wales was 1 in 40. By the last census (of 1821), the yearly mortality had fallen to 1 in 58, nearly one-third. The rate of mortality is of course not equal throughout the country. According to Dr. Hawkins, this is mainly influenced by the proportion of large towns which any district or county contains. The lowest well-ascertained rate of mortality in any part of Europe is that of Pembrokeshire and Anglesey, in Wales, where only one death takes place annually out of eighty-three individuals. Sussex enjoys the lowest rate of mortality of any English county: it is there 1 in 72. Middlesex, on the other hand, affords the other extreme, 1 in 47; yet here, where the rate of mortality is higher than in any part of England, great improvements in the mean duration of life are taking place; for in 1811, the mortality was as great as 1 in 36. Kent, Surrey, Lancashire, Warwickshire, and Cheshire, are the counties where, next to Middlesex, the deaths are most numerous. The three last named counties enjoy many natural advantages, but these are more than counterbalanced by the number and density of their manufacturing towns. It is a circumstance well worthy of note, that the aguish counties of England do not, as might have been expected, stand high in the list. In Lincolnshire, the rate of mortality is only 1 in 62. Dr. Hawkins hesitates whether to attribute this to the large proportion of dry and elevated district which that county possesses, or to the exemption of fenny countries generally from consumption. We are strongly inclined to suspect that the latter is the true explanation of the fact. The notion was originally thrown out by the late ingenious physician, Dr. Wells, who even went so far as to advise the removal of consumptive patients to the heart of the Cambridgeshire fens, rather than to Hastings or Sidmouth.

The author goes on to remark, "that the decline in the mortality is even more striking in our cities than in our rural districts. While the metropolis has extended itself in all directions, and multiplied its inhabitants to an enormous amount,—in other words, while the seeming sources of its unhealthiness have been largely augmented, it has actually become more friendly to health." In the middle of the last century, the annual mortality was about 1 in 20. By the census of 1821, it appeared as 1 in 40; so that in the space of seventy years, the chances of existence are exactly *doubled* in London,—a progress and final result, adds the author, without a parallel in the history of any other age or country. The high rate of mortality in London about the year 1750, exceeding considerably that of former years, has been attributed to the great abuse of spirituous liquors, which were then sold without the very necessary check of high duties. One of the results of these statistical investigations which, *à priori*, we should least have been prepared for, is the uncommon healthiness of Manchester. The rate of mortality there at the present time does not appear to exceed 1 in 74. Drs. Percival and Ferrier, by their introduction of certain police regulations, particularly with respect to ventilation, may justly claim the merit of this great improvement.

The mean duration of human life has been experiencing, on the continent of Europe, changes similar to those of our own country, but very inferior in degree. As far back as the fourteenth century, M. Villermé has calculated, from some manuscript documents, that the mortality in Paris was about 1 in 17. About the middle of the last century, it was about 1 in 25 in Paris, and 1 in 29 for the whole of France. At the present time, it is 1 in 32 for Paris, and 1 in 40 for the whole country. Statistical returns having lately attracted great attention on the continent, the author has been able to institute a very curious comparison between this and other countries in respect of mortality. "The annual proportion of deaths at Montpelier was greater thirty years ago, and is greater at present, than in London; and although the mortality of great cities is usually much larger than that of provinces or counties, yet the mortality of London is exactly the same at present as that of the department of the Herault, the southern, fertile, and long supposed most salubrious district of France, of which Montpelier is the capital." The custom of sending consumptive invalids to the south of France, was reprobated so long ago as 1792 by a German author of the name of Finke, in a work on medical geography. He declares that the cutting winds of those quarters annually destroyed many of those who were wandering in quest of a milder sky. The annual mortality of Nice, a small town enjoying a factitious reputation of salubrity, is as high as 1 in 31; Naples, 1 in 28; Leghorn, 1 in 35. If we take the other great cities of Europe indiscriminately, their inferiority in respect to the value of life, even when compared to the manufacturing towns of England, is singularly great. In Paris, for instance, as we have seen, the annual deaths are about 1 in 32; at Berlin, 1 in 34; at Madrid, 1 in 29. Rome, Amsterdam, and Vienna, are last in the scale of salubrity. In Rome the deaths are annually 1 in 25; at Amsterdam, 1 in 24; at Vienna, 1 in 22. The inhabitant of London, therefore, has nearly twice as good a chance of living as the burgher of Vienna. The farther we extend our researches, the more clearly it appears that Great Britain is at present the most healthy country in the world, and that for the last fifty years it has been gradually tending to that point. The country which approaches most nearly to us in respect of salubrity is the Pays de Vaud, where the mortality is 1 in 49. Yet, even here, how great is the difference! the average annual deaths throughout England and Wales being only 1 in 60. Sweden and Holland lose annually 1 in 48; France, 1 in 40; Prussia and Naples, 1 in 34; Wirtemberg, 1 in 33. "It is remarkable," says the author, "that the superior value of life in Great Britain is not confined to any particular districts or classes of individuals. To whatever point we turn our view, the advantage is still the same. The man of affluence, the pauper patient of the hospital, the sailor and the soldier in active service, the prisoner of war, the inmate of a gaol, all enjoy a better tenure of existence in this country than in any other of which we have been able to consult the records." It has long been the fashion, both at home and abroad, to exhaust every variety of reproach on the climate of our country, and particularly on the smoky atmosphere of London; yet, in point of fact, the most favoured spots in Europe—places selected as the especial resort of invalids—the very fountains of health and strength—are far more fatal to life than even London itself, with its bad air and worse water.

The fourth and fifth chapters of Dr. Hawkins's book are entitled Medical Statistics of Countries and of Cities. They contain a vast number of curious particulars, but very disjointed. We cannot avoid hinting to the learned author, that a subdivision of these chapters, or rather a new modelling of them, having reference rather to the subject discussed, than (as now) to geographical situation, would be an improvement. We will glean, however, whatever appears likely to interest the general reader. Speaking of France, the author for the first time draws the reader's attention to the important principle which is afterwards more fully illustrated and developed—the intimate relation between average mortality and domestic comforts. The smallest mortality occurs in those districts of

France where wealth and its concomitants, ease and happiness, most abound, as in the departments of the *Calvados*, of *L'Orne*, and *de la Sarthe*. In them, one in fifty only die annually; and of a given number of individuals, scarcely one-fourth die before five years of age; one-half die at forty-five; one-fourth live beyond the age of seventy. Let this condition of a prosperous district be compared with that of the less fortunate departments of *L'Indre* and *Le Cher*. In these two poor departments one-fourth of the population die before completing their *first* year; one-half die between the ages of fifteen and twenty; and three-fourths are gone before reaching the age of fifty-five. The general result is, according to M. Villermé, whose calculations are believed to be tolerably correct, that in the wealthy departments of France, life is, on the average, protracted twelve years and a half beyond its course in those which are poor.

The mortality in Prussia is now about 1 in 35. During the years 1816 and 1817, when high prices prevailed, the mortality increased and the births diminished. But the cheapness of provisions since 1819, coupled with improvements in the medical and moral institutions throughout the country, have caused a great change. According to the present annual increase, M. Dupin calculates that the population may double itself in twenty-six years. The number of marriages is greater here than in other countries, a circumstance probably attributable to the great facility of obtaining divorces, which, in 1817, allowed one pair in thirty-seven to change partners. In the same country, one birth out of every thirteen was illegitimate; but we are happy to add, that the number both of illegitimate births and of divorces is now experiencing a gradual diminution.

Austria would not appear, from statistical inquiries, to be a very thriving country. Professor Kudler informs us, that the population advances so slowly, that it would take about two hundred years to double the population of its several provinces. In Holland the value of life is on the increase. The mean average for the whole kingdom is 1 in 48. "Mr. Quetelet has found that the mortality is the greatest in those parts of his Netherlandish majesty's dominions which are the most populous, the nearest to the sea, and where the ground is most low and marshy." We should not have thought this worthy of especial mention, had we not observed in several instances that statistical results were not always the simple suggestions of reason and common sense.

Sweden has long been celebrated for its love of statistics, and we have accordingly many curious notices regarding that country. Among the deaths in 1823, only five persons exceeded the age of one hundred years, of whom one was a man, and four women. The marriages were nearly 24,000, amongst which ten men married for the fourth time, one for the fifth, and one for the sixth time! Upwards of 1400 women bore children from the age of forty-five to that of fifty. Fifty-three women had borne children beyond the age of fifty. Among nearly 100,000 women in labour, 1400 had twins, and twenty-seven had triplets.

Dr. Hawkins has not been very bountiful in his statements regarding those countries where slavery exists; and we regret that he has not paid attention especially to the statistics of our own West India colonies. He will find, we apprehend, some very curious results when he addresses himself, as we hope he will, to this rich field of investigation. We extract what he says about the Isle of Bourbon. "The French colonists (or whites) increase very rapidly, not by the arrival of new comers, (of whom the annual average is only a fiftieth,) but by the great excess of births over deaths, which is in the proportion of about 9 to 5. The progress of the slave population, on the other hand, is very different, and marks well the intimate connection subsisting between social position and physical development. From 1818 to 1824, it has diminished by one-sixth, and is decreasing more and more rapidly, so that the proprietors are anticipating, at a short distance of time, the necessity of cultivating less ground, and of abandoning the productions which require particular labour. So great is the mortality, and so few the births, that while the whites are annually gaining nearly two per cent., the black population is retrograding at the rate of *three* per cent. annually, in spite of the advantages of a climate so much more congenial to the African than to the European temperament." There exists in the Isle of Bourbon an intermediate class, the free blacks, holding a condition in society nearly similar to that of the whites. It is interesting to observe the superior longevity and reproductiveness of this class of persons, attributable, beyond all doubt, to the favourable influence of liberty. The mortality in this portion of the population is even less than that of the French colonists, because, with the advantages of comparative prosperity, they combine the occupation of a native soil.

We have already had occasion to notice the increased value of human life in rural districts compared to what holds good in cities. This difference is principally felt in the first five years of life, and again between the ages of twenty and fifty. It is worthy of notice, that after the age of fifty, the proportion of deaths in London is smaller than in the country.

"In all cities," says the author, "a large portion of disease and death is to be assigned to the constant importation from the country of individuals who have attained to maturity; but having been previously habituated to frequent exercise in a pure atmosphere, and to a simple regular diet, are gradually sacrificed to confined air, sedentary habits, or a capricious and over-stimulating food." Some recent experiments by Dr. Baron, of Gloucester, seem to throw light on this statistical fact. They shew that, in the lower orders of animals, the loss of their open range and natural nourishment has a tendency to disorganize and destroy. A family of seven young rabbits were placed in a confined situation, and fed with coarse green food, such as cabbage and grass. In about a month they began to die. The primary step of disorganization was evinced in a number of transparent vesicles studded over the external surface of the liver. In a second rabbit, (which died nine days after) the disease had advanced to the formation of tubercles on the liver. The same thing happened to a third and a fourth. A considerable number of hydatids were attached to the lower surface of the liver in this latter case. Dr. Baron now removed the three remaining rabbits to another situation, dry and clean, and restored to them their proper and accustomed food. They all did well.

Some of the statistical notices regarding Paris are worthy of being recorded. The difference in the mortality of the poor and rich quarters of the town is quite surprising. Villermé calculates, that where there are 50 deaths in the rich arrondissement, there are 100 in the poor one. The poor district, however, produces more children than the rich, in the proportion of 32 to 26. Notwithstanding this, there are not, at five years of age, more children in the one than in the other, because, though more children are born, they are not so well nourished. The number of illegitimate children is immense, and seven out of nine are abandoned by their parents. Of infants thus abandoned, one half perish in the first year of their existence.

"Reviewing, on one side, the great political, moral, and physical events which have occurred at Paris during a succession of years, and, on the other, the progress of its population, Villermé has ascertained that whenever from any cause the people have suffered, not only have deaths increased, but births have decreased, and the mean duration of human life been shortened. In periods of prosperity he has found results directly opposite to these." Such is the connection which Statistics prove to exist between domestic and national calamity.

"It was formerly estimated that one-third of the inhabitants of Paris died in the public hospitals. The more recent inquiries of Dupin have ascertained that *one-half* of the deaths in Paris take place in the hospitals and other asylums of charity. *Not a fourth part of the inhabitants are buried at private cost.* We cannot avoid pressing these facts on the notice of our legislators, with reference to the proposed plan of supplying subjects for anatomical schools. It is obvious that France and England are utterly at variance with each other on the subject of respect for the dead, and no argument drawn from the practice of one country can be made applicable to another, whose feelings on this great principle of human action are so radically opposed. We are sure no one would wish to see the prejudices of Englishmen on this question (if prejudices they be) in any degree altered. They are associated with all that is most delightful in the English character, strong family attachment—the pride of ancestry—the honourable ambition of founding a family. It is not by any means our intention, in thus expressing ourselves, to discourage parliamentary interference in the regulation of anatomical pursuits, but we would only recommend caution in adopting the customs of our scientific but light-headed neighbours.

Vienna affords some instructive lessons. The annual mortality of this city, which in the middle of the last century was about 1 in 20, is even now 1 in 22. The excessive spirit of legislation, the dread of novelty, the restrictions imposed on the medical profession, and certain other *political* causes, have occasioned this retardation in the natural progress of the city. The overweening *paternity* of the government interferes with the trivial concerns of the citizens, in the same manner in which an arbitrary and untaught father sometimes restrains the useful impulses of his children, while he permits an easy vent to their baser propensities.

Prague, the capital of Bohemia, is both a larger and more healthy place than Vienna. Many instances of great longevity are recorded as occurring there, and in this respect the ladies appear to have greatly the advantage over the gentlemen. Another curious fact, too, appears from the statistics of Prague. Neither bachelor nor spinster has, for an average of several years, exceeded the age of 95, whereas several *married* persons have lived to see 115. Individuals earning a laborious subsistence are also longer lived at Prague than the nobles and the men of wealth. This, however, adds the author, is an extreme and insulated fact, and does not militate against that general principle which we have already laid down, and which a variety of evidence appears to establish, the *conservative* tendency of prosperity.

Dr. Hawkins is scanty in his statistical notices of Rome, which we the more regret, as the subject is one of such commanding interest to the English traveller. From a recently discovered fragment of Cicero, we may gather that, from the earliest times, the neighbourhood of Rome has been unhealthy. The population of Rome appears to have gradually decreased during the time of Buonaparte. Between 1800 and 1810, the imperial city lost 27,000 of its population. Since the peace of 1815, it has gained an accession of almost 10,000. The present annual mortality is about 1 in 25. The author gives it as his opinion that the force of the aguish disposition of Rome might be considerably weakened by steady and well directed efforts, supported by a proportionate capital, but expresses his fears that such a combination of circumstances will not readily meet at Rome. We are almost tempted to doubt this favourable view of Roman susceptibility. There seems to be something in the geology of the Campagna di Roma which will for ever prevent it from being other than it is, the focus of malaria—the favourite haunt of ague and fever!

The declining commerce of Amsterdam has led to an increase in its rate of mortality. In 1777, this was one of the most healthy cities of Europe, as it was also one of the most flourishing. The annual deaths were then only 1 in 27, a very low rate for that period. They have now increased to 1 in 24. Stockholm is another declining capital. In a recent year, this city exhibited the singular phenomenon of an excess of deaths over the births, to the amount of 1439, a symptom, adds our philanthropic author, which it is painful to observe in a brave and industrious people. Drunkenness appears, here, as well as at Berlin, to have a large share in the deterioration of life. Baron Von Fahnenberg, on whose authority the statement is made, affirms, that this vice destroys the happiness and prosperity of Sweden more effectually than any war has ever accomplished.

The following interesting statements are made with reference to the different degrees of mortality in the white and black populations of North America. At New York, the mortality of the whites, on an average of the seven years, from 1820 to 1826, is 1 in 40. The annual mortality among the blacks alone is, on the same average, as high as 1 in 19. At Philadelphia, the blacks die at the same rate of 1 in 19 annually, while among the whites 1 in 34 only are carried off. At Baltimore, the condition of the black population improves; 1 in 33 is here the annual mortality of the blacks; 1 in 39 of the whites. In this city the *slaves* enjoy a longer existence than the *free blacks*, which the author considers to be owing probably to the intemperance and improvidence of the latter when left to their own control.

The sixth chapter of Dr. Hawkins's book is dedicated to the statistics of General Hospitals; and he first offers some reflections on the mortality of hospitals, on the causes which mainly influence it, and on the measure of importance which may properly be attached to this branch of the investigation. He obviously considers the statistical reports of hospitals as of the highest value, and quotes in his favour the opinion of Mr. Milne, one of the most eminent writers of the present day, on the probabilities of life. Such reports, he says, throw the greatest light on the causes of the increase and decrease of different diseases, and of the great differences which are found to exist in the degrees of mortality in different situations, and among different classes of the people. The most extensive *private* practice, says Dr. Hawkins, is inadequate to supply so copious a store of evidence in the history of disease. The author combats the often-repeated paradox, that the deaths in an hospital become more numerous as its discipline improves, and the skill of its medical officers increases. The conclusion to which he comes is, that in every city the mortality of the hospitals has declined in proportion to the increase of prosperity, and to the diffusion of knowledge; and wherever it maintains a *high* standard, there the lower orders will correspond in their condition of want and debasement, and the medical profession will be seen to occupy but a low place in public estimation.

Next to the influence of *national* causes, says the author, the mortality of hospitals is most affected by position and internal economy. These circumstances appear more powerful than even the various merits of *practice*. The doctors have certainly been very shamefully treated by the world. Statistical reports can be brought to prove, that even the worst of them do not thin the population to the extent that their enemies have often slanderously imputed. "*Mortality*," says Dr. Hawkins, (page 78) "is seldom to be assigned to the influence of bad practice, which probably does not often destroy life. An accomplished friend made particular notes on the comparative mortality under three physicians in the same hospital. One was *expectant*, one *tonic*, the other *eclectic*: the mortality was the same, but the length of the disorder, the character of the convalescence, and the chances of relapse were very different."

Ventilation appears to be the circumstance on which the health of hospitals mainly depends. In former times, when a singular prejudice, or indifference to ventilation existed, the mortality was often excessive, and the loss of lives after surgical operations, in particular, dreadful. In the year 1685, the proportion of deaths to cures at St. Bartholomew's and St. Thomas's Hospitals, was about 1 in 7. It is curious to observe that the reports of St. George's Hospital, so recently as 1825 and 1827, afford a nearly similar result, 1 in 9: but it is reasonable to believe that much of this is owing to the improper construction of that hospital. In some of the other hospitals of London, the mortality does not exceed 1 in 15. The provincial hospitals of England are lower in respect of mortality than the metropolitan, and it is well worthy of consideration, whether this be not owing (in part at least) to the smaller size of the buildings. That the congregation of a large mass of persons has a tendency to promote the formation of disease, and to propagate and exasperate it when formed, seems a reasonable supposition. On this account it will, perhaps, be found that a city is better served by several small hospitals, than by a few large ones. Added to which are the greater conveniencies thus afforded to transport, visits, and study.

Many curious particulars are recorded of the mortality of the several hospitals of Paris. We can only find room for a few of the most interesting results. The mortality in the hospitals, taken all together, was 1 in 8. Between the years 1770 and 1780, the mortality at the Hotel Dieu was about 1 in 4 of all admitted. We cannot wonder at this, when we learn, by the testimony of an eye-witness, that he often saw on the same bed, a dead body lying by the side of two dying patients, and of one convalescent. These things are now happily changed, yet the mortality in the same hospital in 1822, was nearly as high as 1 in 6. In the Charité, another general hospital, the mortality was in that year 1 in 5. As in England, the provincial hospitals of France enjoy a greater share of healthiness. The deaths in the principal hospital of Lyons are only 1 in 11. At Montpelier, 1 in 10. Berlin has nothing to boast of in respect to its hospital statistics. The mortality of its great general hospital, the Charité, on an average of twenty years (from 1796 to 1817) was 1 in 6. Nearly the same rate holds good in the hospitals of Vienna, Pest, and Dresden. Munich, otherwise happily circumstanced, is described as possessing one of the most modern and best regulated hospitals in Germany. This will explain the comparatively low rate of its mortality, 1 in

9, the lowest in fact of any hospital of the same dimensions in Germany. The inquiries of the author into the hospitals of Italy, shew that a very similar ratio prevails there, varying from 1 in 6 to 1 in 10. Registers of deaths appear to be admirably kept in those parts of Italy which are under the Austrian government.

The author next proceeds to remark upon the happy change which has supervened within the last hundred years in the fate of Lying-in women. In 1750, at the British Lying-in Hospital, 1 woman died out of 42; in 1780, 1 died in 60; and at the City of London Hospital, in 1826, the deaths were only 1 in 70. At the Dublin Lying-in Hospital, the most magnificent institution of the kind in the world, the proportion of women dying in childbed, on an average of seventy years, has been but 1 in 89. In this respect we are greatly in advance of our continental neighbours. At Stockholm and at Paris in 1822, the deaths at the lying-in hospital were 1 in 29. At Berlin, on an average of the ten years between 1807 and 1817, 1 case was fatal out of 45. The mortality there had once been as high as 1 in 32.

Few things are more puzzling than the statistics of Still-born children. We may give our readers the general results. Still-born children are more frequent in towns than in the country—amongst the poorer classes than the affluent—amongst the unmarried than among married women. Amongst the most unfortunate of the former, at Paris, 2 out of 7 are born dead. If we take a whole town, as Göttingen, only three per cent. of the children born in marriage are still-born; but as many as fifteen per cent. of those born out of wedlock. At Stuttgard, it has been remarked that the number of the still-born augments nearly in the same proportion as that of the illegitimate births. All this is intelligible enough; but how shall we account for the number of still-born children at Strasburg, which is as 1 in 12 of all the births, while in London it is as 1 in 24, and in Sweden only 1 in 40?

By far the most interesting fact which statistical inquiries have ascertained is the diminution which has been gradually taking place in the mortality of Infancy. Beneath the thin layer of brilliant colouring which writers have spread over the nations of antiquity, lurks a deep and dark corruption, no where more perceptible than in their astonishing indifference towards their own offspring. It is beautiful to contemplate the improvement which modern times exhibit in this point of duty. Every succeeding century, and every advance in the road of Christianity, are marked by increased attention to the physical treatment of children, and by a diminution in their mortality.

In most of the states of Greece, infanticide was not merely permitted, but actually enforced by law. The Romans were not less unrelenting in their treatment of infants. Among the Chinese, the exposure of infants is notorious; and until lately the same custom prevailed to a great extent among the Hindoos. The boasted triumphs of Greece and Rome record no provision for the friendless. The great object of their buildings and institutions was to pamper the vices and flatter the ambition of a few selfish and proud individuals. The worldly or civil influence of Christianity, adds our author, cannot be better traced than in considering the sudden growth of asylums for the sick and destitute, which accompanied its progress, and which may be sought in vain in the history of antiquity. *Foundling Hospitals* gradually flowed from the admirable spirit of charity thus introduced, and whatever we may think of their policy, the feeling which created them can only excite respect. It is now well understood that the facilities which they afford corrupt the maternal instinct and offer a premium to seduction. It is but little they effect towards the preservation of the children. Such is the frail tenure by which infant life is held, that remitted attention, even for a few hours, is full of danger, and the desertion of an infant by its mother is, in point of fact, nearly equivalent to its destruction. Wherever foundlings are received indiscriminately, the mortality appears to be beyond the control of all attention or skill. One half of the children received at the Foundling Hospital of Vienna died prior to 1810. Under the new system of nursing *in the country*, the mortality has fallen to 1 in 4. In the foundling hospitals of Sweden, Russia and Spain, matters are even worse, so that we may congratulate ourselves on the exemption of this country from these plagues. It is due to the Foundling Hospital of London to say that it is conducted on such principles as prevent all injurious effects from it.

To estimate properly the beneficial change which modern times have effected in the value of infantile life, we must turn from foundling hospitals to domestic fire-sides. We are indebted to Sir Gilbert Blane, Dr. Casper, and Mr. Robertson of Manchester, for the facts which illustrate this important principle. At Warrington, the deaths under ten years of age were formerly fifty-five per cent. They are now reduced to forty-four. A similar improvement has taken place in Glasgow, where it appears that in a given number of inhabitants, the deaths among children have decreased to two-thirds of what they were in former periods. Much of this is to be attributed to better nursing, and the general increase of prosperity in the country; but a great deal is unquestionably due to the progress of medical knowledge, and, above all, to the general diffusion of vaccination.

The author next adverts to the subject of Insanity, regarding which the following interesting statements are made. In almost every country, except France, the number of male lunatics exceeds that of the females. The exception there is attributed to the more active part which the French woman plays in society. Casper contrasts the decent deportment and dress of the women in English asylums, with the coquettish manner and absurd toilette of the female inmates of a French lunatic hospital. Insanity is one of the few important disorders which increase with the progress of civilization and refinement. "It is, however, a solitary mourner in the train of prosperity, and is lost in the crowd of blessings which attend the extension of education and affluence." No estimate which has been formed of the comparative prevalence of maniacal disorders in this and other countries can be at all relied on, in the present state of our knowledge. Any public calamity, such as a national scarcity, has a tendency to increase the victims of this malady.

With respect to Prisons, the author notices the curious contrast in the mortality of those of London and Paris. The highest rate of mortality any where known among adults is at the Depot of Mendicity of St. Denis, at Paris. On the other hand, it is stated as a curious fact, that the galley-slaves of France only die at the rate of 1 in 49, while that of the whole French nation is as 1 in 40. Some very curious and instructive facts are recorded regarding Suicide. It is now clearly proved, that the charge so often made against Englishmen, of proneness to suicide, is most unfounded. Dr. Burrows has the merit of having first vindicated our country from this heavy slander. A recent report of Mr. Higgs, the coroner for Westminster, has set this matter quite at rest. During thirteen years (from 1812 to 1824), the total number of coroner's inquests in Westminster was only two hundred and ninety, which, if *trebled,* would still shew an advantage over the great cities of France and Germany. Some curious instances are given of the spread of suicide by imitation; and a hint is thrown out that this, as well as other evils, are really propagated in this country by the long and laboured details of crime with which our newspapers teem. There can be little doubt that the incendiary of Westminster Abbey might have brooded long over his supposed grievances without any overt act of malice, had he not read of Martin, and the burning of York Minster. Roman Catholics and Jews are stated to be less disposed to commit suicide than the members of Protestant communities. The following singular statement is made, (p. 171):—

> A *suicide club* is *said* to have existed lately at Paris, but the members were not likely to become numerous: they were twelve, and the leading regulation directed, that one member should be annually selected to put an end to himself.

We shall not detain our readers by the statistics of different disorders, though many curious particulars regarding the increase and decrease of maladies are given, but proceed to the statistics of *Climate* in relation to health and disease, constituting the thirteenth chapter of the work. Without denying the influence of climate, the chief aim of the author is to shew, that its importance has been greatly overrated. The power of accommodating himself to every variety of climate is one of the most important prerogatives of man; and in Europe at least, the maladies of the individual depend much more upon his own habits and condition, than upon the varieties of climate. Imprudence is the great source of the evils under which Europeans suffer in tropical countries. It is no doubt true, that a greater share of health and longevity is enjoyed by the northern than by the southern countries of Europe and America; but the superiority of the former in civilization, prudence, and good government outweigh, in the author's mind, the difference of latitude. There is assuredly a great deal of truth in this, but the influence of climate cannot be disregarded, and indeed many of the statements in the work amply prove its extent. The unsightly goitre appears to acknowledge locality alone for its cause; and if other proofs were wanting, the bills of mortality at Sierra Leone would be decisive of the question. In the tropical fever of Jamaica, in 1808, two hundred deaths are reported to have occurred out of four hundred and ninety-four cases.

The influence of season upon mortality is briefly touched upon. The order of the seasons, in respect of salubrity, varies in different climates. In London, at the present time, the autumn is the most healthy season. Next follow the summer and winter. The least healthy season with us is the spring. At Paris, also, the greatest mortality occurs in April; the lowest in July. In Italy, on the other hand, and at Montpelier, the order is reversed. March is there the most healthy month, and the autumnal season is chiefly to be dreaded. It may be observed, generally, that in all healthy districts the winter and spring are the seasons productive of greatest mortality.

> When we speak of a healthy climate, it is gratifying to reflect that in most instances it is man himself who has in a great measure created these climates of health. Twenty centuries ago, England, France, and Germany resembled Canada and Chinese Tartary, countries situated like Europe, at a mean distance between the equator and the pole. Macchiavelli, in his early age, seems to have anticipated this truth: he remarks, in his quaint language, "Unhealthy countries become wholesome by the multitude of men who inhabit them; who at the same time are occupied in cultivating the earth, and who make the earth sane: the fires which they kindle purify the air: these advantages nature herself does not produce."
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> It is only by constant efforts of industry that the salubrity of any spot is maintained: when these are relaxed, or when prosperity and civilization decline, the seeds of disease are immediately deposited in the earth. The aguish disposition has been observed to increase at Rome in the same proportion that its population has diminished. On the other hand, it is well known that the climate of the United States has been remarkably improved by draining, cutting down trees, and the operations of agriculture; and that spots which were impracticable, or fatal to the early settlers, at present afford a comfortable residence.

After several statements, confirming the great principle already alluded to, that competence, comfort, and exemption from mental anxiety and bodily fatigue, are the great preservatives of health and strength, the author proceeds to the statistics of Trades and Professions. This is a very interesting branch of the subject; and we regret to find it so briefly discussed. With reference to the insurance of lives, it is obviously of the highest importance. The prevalence of consumption among masons is alluded to, but the exemption of tanners from that scourge of mankind seems to have escaped the author's attention. The diminished mortality of soldiers and sailors, under the present improved regulations of the service, is clearly pointed out; but it is added, that a portion of the good health enjoyed by our army and navy is due to the influence of *moral* causes, such as national spirit, and general success. Their operation on the health of soldiers was strikingly evinced in the French armies, during the (to them) disastrous campaigns of 1813 and 1814. The amount of disease augmented with every fresh failure. Soldiers are stated by the author to enjoy a better prospect of longevity than sailors, attributable, as we may reasonably suppose, to their being less exposed to severe labour, the inclemencies of weather, and privations of good nourishment.

The statistics of the sexes afford some curious results. The relative numbers of the sexes are the same in all parts of the world,—namely, at birth, twenty-one males to twenty females, but as the mortality among males during infancy exceeds that of females, the sexes at the age of fifteen are nearly equal. A late French writer, M. Giron, thinks himself warranted in the opinion, that agricultural pursuits favour an increase in the male, while commerce and manufactures encourage the female population. There exists throughout the world considerable variety in the proportion of births to marriages, but, upon an average, we may state it at about four to one. It has been uniformly found, however, that improvements in the public health are attended by a *diminution* of marriages and births. The great principle is this: as the number of men cannot exceed their means of subsistence, *if men live longer, a less number is born*, and the human race is maintained at its due complement with fewer deaths and fewer births, a contingency favourable in every respect to happiness. The author illustrates this very important principle by the population returns both of England and France.

Having pointed out the many varieties in the distribution of mortality, Dr. Hawkins concludes the volume by a brief notice of the circumstances which, in our own country, have rendered its diminution so conspicuous. They are, improvements in medical practice and in the construction and general economy of hospitals;—the increase of commercial and agricultural industry—improvements in our clothing and diet; but, above all, the spirit of English liberty.

> We may easily conceive the different constitution of body and of mind which is likely to grow upon the unemployed inhabitant of a decayed city, who gloomily wanders without an object through silent streets whose pavement is choked with grass; and upon the active citizen who feels himself a constituent member of a flourishing community, and who is attracted on all sides by invitations to the exercise of his faculties.
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> It is indisputable, that the average proportion of deaths in England and her cities is less than that of any other country of Europe. And it may be added, that the powers of body and of mind are preserved to a late period in higher perfection here than in other countries: nowhere are the advances of age so slowly perceived, and nowhere so little manifested on the exterior. An analogous condition of health and vigour may be also observed in our animals and in our vegetation; and if it should be replied, that this excellence is owing to the care bestowed on their culture, the answer applies equally to the human being, on whom more attention is here bestowed, and who is really an object of greater value here than elsewhere.

It is delightful to find the author thus displaying at the same time his science and his patriotism, and so elegantly expressing those feelings of national pride which his statistical investigations have fostered.

We take our leave of him with the respect which is due to the scholar, the physician, and the gentleman; and would strongly encourage him to persevere in that field of study in which he has first broken ground, and which promises to yield him hereafter a rich harvest of honourable reputation.

Illustrations of Nature.

To the Editor of the QUARTERLY JOURNAL OF SCIENCE.

SIR,

In pursuing the Illustrations of Nature, bats and their allies follow in course the manupeda, or monkey type.

Objections have been raised to the separation of these animals from the carnivoræ, as not worthy (from the unimportant part they are said to play in the general economy of nature) to be dignified as an independent section. What may be their comparative importance I shall not presume to determine; but thus far I will fearlessly assert, that if their natural characters permitted their disjunction from the Linnæan Primates, those same characters should never have allowed their conjunction with the Predaceæ and Feræ. I would also with deference submit, that systems should succumb to natural characters; and natural groups never be forced or kept in unnatural associations; either from their confined number, (which our ignorance alone may limit,) or from any prejudice we may form of their value in the system of creation. Thus man, justly freed from his unnatural allies, the apes, the monkeys, and the bats, is now confessed the single species of a single genus, in the single order of his class and reign: and thus the Ornithorynchus was rightly disjoined from all other beasts, even when the solitary example of its type and order: thus also, bats might with advantage be considered a separate section, even were they in number less; but those already known are far from trifling.

Naturalists acquainted with the progress of zoology will scarcely object to the subsections of the subgenera of the former genus Vespertilio, being now esteemed as *genera,* and the larger groups as *kinds;* especially when they reflect that the signification of this term has ever been varying in extent, from its first use, when all beasts formed a single genus, all birds another, all fish a third, and so on. The steps were gradual by which it became limited; and one hundred and fifty such various species of the single genus Vespertilio would be now intolerable. Illiger, Geoffroy, the Cuviers, Leach, and others, have already, more or less, authorized these subdivisions; from their works, aided by the examination of specimens, the concluding tabular synopsis has been compiled.

In the present essay I have fallen almost unconsciously into a longer criticism than was purposed of their earlier arrangement; this, however, will not be without its use, as it may tend to settle an historical point which appears in general to have been misunderstood, and frequently involved in unaccountable confusion; for the error even to this day prevails, as may be shewn by its repetition in a zoological work, published long since the materials of these papers have been ready for the press.

I have the honour to remain,
Yours obediently,
GILBERT T. BURNETT.

May 3*d,* 1829.

Illustrations of the Alipeda (*Alipeds*), *or Bats and their allies; being the arrangement of the Cheiroptera, Volitantia, or wing-footed Beasts, indicated in Outline.*

BATS and their natural allies, a well marked series of animals, though always notorious, were formerly so little understood, that a structure highly characteristic was deemed ambiguous; a curious error, which their nocturnal habits favoured, leading not only the vulgar but philosophers astray. The Ataleph of the Hebrews, the Nycteris of the Greeks, and the Vespertilio of the Latins, would appear for ages to have ranked as birds: and thus together we find associated "the ostrich and the bat" [Jonstonius de Avibus]; "the crow, the bat, and the lark," [Geof. Linocier, Hist. des Oiseaux]; "the sparrow, the partridge, and the quail, the starling the ostrich and the bat," [Arch. Simson].

Aristotle and Pliny are generally stated thus to have believed and taught; but their authority, as well as that of the Pentateuch, has been cited in this matter, on very insufficient grounds. It is true that, as *fowls*, we find enumerated "the heron, the lapwing, and the bat," [Levit. xi. 19.]; but the locust and the beetle are likewise named; and the Stagirite especially quotes as his examples the "hawk, the bee, the beetle, and the bat," [Ζωων ἱϛορία, A. I. 5]; indeed he avoids the word ὄρνις, expressly using πτηνὰ; as Pliny in like manner wrote *volucris*, expressly avoiding *avis*. This uniform tenor in their works demonstrates that the zoological distributions of the ancients were much more extended in their signification than similar terms are accepted in modern times; their *fowls*, πτηνὰ, *volucres*, probably included all animals that fly, of which *birds* were but a section; as quadrupeds, τετράποδα, all that possessed four legs; amongst which are found the tortoise, the crocodile, and the frog, examples immediately in point.

That error, which unjustly was attributed to the ancients, the naturalists of the middle ages themselves incurred; for, without such qualifying circumstances, Charleton arranges, in one section of his birds, "the estridge, the emeu, and the bat;" in the Ornithologia of Conrad Gesner, we find assembled, "turdus, vespertilio, et ulula:" Belon enumerates, as birds of prey, "l'aigle, le busard, le faucon, l'hibou, et la souri-chauve;" and Merret places together as allies, "the eagle, the hawk, the falcon, the chough, the crow, the bat," and others. Albertus Magnus and Franzius associate the "owl and the bat;" Jonston and Konig both call them carnivorous birds, and class in one family the "ostrich and the bat" [aves carnivoræ mediæ naturæ]; and of the genus vespertilio, the former gives two species, v. lactans and v. pullus [Theatr. Univ. de Avibus]. Again, Aldrovandus, after having stated that some skilful naturalists had doubted whether bats were quadrupeds or birds, continues thus: "Volucris mihi potiùs quàm quadrupes existimandus vespertilio videtur;" and further, "Nos tamen cum ornithologi et Bellonii exemplo, tum rationibus magis permoti inter *aves*, et quidem nocturnas ponendum existimavimus" [Ulyss. Aldrov. Ornithologia]; thus deeming them not merely 'flying animals,' but birds; as did Wolfgang. Franzius, also, who having discussed the question, —"utrum sit avis an vero mus," concludes the argument in these words:—"Vincit tamen illorum sententia, qui referunt inter aves" [P. ii. c. 23, De Noctuâ et Vespertilione].

Some doubts, however, as to the true nature of the bat, seem often to have occurred; for we find Plato enigmatically referring to it as a bird that is not called a bird; and Aristotle himself, although in one place he allies αλωπηξ καὶ νυκτερὶς, in another, when jealously scanning its structure [2. iv. 13], he observes [14], τον αὐτον δὲ τρόπον καὶ ὁ στρουϑός ὁ Λιϐυκός τὰ μεν γαρ, ορνιθος ἔχει· τα δε ζώον τετράποδος. Macrobius, in like manner, describes it as an animal of an "uncertain kind;" and Aldrovandus, though he decides it to be a bird, entitles the chapter in which he treats of the ostrich and the bat, "De avibus mediæ naturæ:" by some it has been "placed in the middle betwixt two kinds, as having something of birds and beasts," and thus Nonius defines it, "inter avem et murem species;" while Victorinus called it "avis sorex," and Valla "semi-mus."

Still the current doctrine of the middle ages,—nay, even to the end of the seventeenth century,—was, that the "bat is a small bird;" and thus Topsell excluded it from his "Historie of Beastes;" while ornithologists classed it among the birds, "avis impura," "avis fœda, vultu humano."

The progress of this error it would be futile to trace further; its extension is well marked by Holland's unintended perversion of the text of Pliny: the sixty-first chapter of the tenth book he thus prefaces, and quaintly enough translates, "What bird alone bringeth forth a living creature and feedeth it with milk." "The reremouse, or bat, alone, of all *creatures that fly* [volucrum], bringeth forth her young alive; and none but she of that kind hath wings made of pannicles, or thin skins; she is the only *bird* [volucris] that suckleth her little ones with her paps, and giveth them milk."

Wotton, so early as the reign of our sixth Edward [1552], truly described, and correctly arranged these animals, positively asserting, in spite of the prejudice that the "bat is a small bird," "Quadrupes est vespertilio," "the bat is a quadruped." "Cauda tam quadrupes quam volucris caret, volat sed cuteis pennis membranave. Horum quoque alæ articulos habent: et volucrum animal parit vespertilio tantum. Dentes habet ut quadrupes, et lacte fœtum nutrit admovens ubera quæ pectori gestat" [Ed. Wotton, Oxoniensis]. Notwithstanding a knowledge of these most decisive characters, the general delusion was but little checked; and it was left for Ray, to whom naturalists are so much indebted, and on so many points, to expunge this error from our systems of zoology. He stated and settled the question with his usual perspicacity. "Vespertilio. Νυκτερὶς. The Bat, or Flittermouse. Hoc animal male a nonnullis avibus accensetur ob alas et volatum; cum nec pennatum sit, nec rostratum, nec oviparum, quæ certæ et characteristicæ avium notæ sunt, ut alia multa omittam. Præter alas enim et volatum nihil fere cum avibus commune habet, sed per omnia cum animalibus quadrupedibus viviparis convenit. Nec major est ratio cur vespertilio quam cur lacerta alata avibus annumeretur" [Raii Syn. Anim.].

Since this period, no doubt has been entertained, among naturalists, as to their first locality; all have followed Ray's example; and Seba, in his splendid work soon after published, expressly calls them beasts, "Hæ bestiæ," in contradistinction to the other tribes. Still the general statement long concurred with popular authority to perpetuate the creed that "the bat is a small bird" [Bailey, &c.], and this the modern Scotch name, *backie-bird*, may sufficiently attest.

The Linnæan classification of bats with men and monkeys was noticed in the illustrations of the last-mentioned type; and the unnatural association of the so called primates, was then objected to; as also in a former essay; q. v. *Journal of Science, No. VI. p.* 392, *No. VIII.* p. 300. Pennant, who rejected this division, said, "vanity would not suffer him to rank mankind with apes and bats;" and later zoologists have followed his example, though doubtless from a better cause. Winged quadrupeds they were termed by Pennant, a less objectionable name than Cheiroptera, or hand-winged beasts, by which it has been lately superseded. The anterior extremities of bats are not hands, and therefore it is to be lamented that hand-winged should be preferred to wing-footed; which meeting a classical translation in Alipes, is not only more euphonious, but also more correct. Thus as manupeda, manupeds, or hand-footed beasts, not unfitly denotes the apes, the monkeys, and the lemurs; alipeda, alipeds, or wing-footed beasts, may in like manner designate the bats and their allies; while quadrupeda, quadrupeds, will comprehend all the true four-footed beasts, of which and the other types more shall be said hereafter.

Bats, even when Linnæus wrote, were comparatively but little known. Six species are all that he enumerates as belonging to the genus Vespertilio, and to this by him was the family confined; for galeopithecus he thought a lemur, and noctilio was misplaced among the glires. Since then they have much increased, both in dignity and importance, for modern researches have added so greatly to their variety and number, that this single genus hath become the common type of several; about forty being already known, and nearly one hundred and fifty species have been described. Our knowledge of these animals, however, is still imperfect; yet we have learnt enough to protest with Pennant against those dental methods of arrangement, which would separate a series so well allied, and scatter the type through several orders of the class of beasts: *e. g.* Brisson took Pteropus from the bats and allied it with the monkeys, and the lemurs from the monkies, and allied them with the bats, &c. &c.

The teeth, from the number and variety of which the characters for orders in the class mammalia of Linnæus, and quadrupeda of Brisson, are chiefly, and in the first place, drawn, although invaluable as subordinate diagnostics, greatly fail, as indeed all single characteristics must, when generalised for the distribution of an entire class. Such schemes when spread on paper, vaunt that imposing feature, great simplicity; but, in the museum and the field, they form no easy task. Nothing would seem more facile than to count the teeth, beginning with the toothless, as the anteater, and passing by the elephant, the stag, and the mouse, to the monkey, the bat, and the seal; until the series terminates with those possessed of the greatest number and variety; as the bear, the mole, and the opossum; yet such is not the case, for we find naturalists the most astute, differing not only with each other on this point, but often with themselves; and if the veterans are at fault, how shall such a plan to the student be a guide?

The teeth are in general described, and with sufficient accuracy for common purposes, as incisors, canines, and molars—*i. e.* cutters, tearers, and grinders; although the canines are often indistinguishable as such, or so situated as to be unfitted for that duty; and the incisors and molars, as in bats, are not unfrequently so jagged and set with points, as to be piercing and tearing rather than grinding teeth. Indeed, in the type now under consideration, the dental system is quite anomalous; for not only do the genera vary extremely in the number and character of their teeth, but the transitions are so obscure, that zoologists greatly differ in their descriptions; and Fred. Cuvier, who has paid much attention to this subject, differs from his predecessors, more than even they differ from each other. Furthermore, the teeth of many appear to be fugacious, and to change so much at different periods, that the same bat has occasionally been two or three times described as a different species, or even genus, according as teeth have been lost from age or accident. Hence only can we account for the discrepancies alluded to: and this has been thought to be the case with some of the proposed subgenera of Rafinesque, more especially as he states the incisors to be *very far apart*, *to be absent*, &c. It must be in the recollection of many that the casual loss of the incisor teeth in the prochilus led to a curious misnomer of that animal; and it is still called the *ursine sloth*, a monument of the impolicy of trusting to any single artificial sign; for had the natural characters been examined, such a mistake could never have occurred. Any character rather than the number of teeth should be resorted to in the arrangement of bats, for in no series of animals are variations, fortuitous and natural, more frequent: hence even subgenera established on these circumstances alone should not be received without jealous scrutiny. From another source the true diagnosis must be drawn, and with an outline of the characters of the type, the races, and the kinds, these observations shall conclude; a synopsis of the genera will be added; at present, to define these latter, would be foreign to my plan, and swell this paper to an inconvenient length: indeed, I am not satisfied that all have been duly ascertained, though such as have been established on creditable authority could not be well omitted.

Type ALIPEDA, Alipeds, wing-footed beasts, (Cheiroptera, Volitantia). Bats and their allies; extremities, especially fore-legs, instructed with pannicles, serving for more or less perfect suspension in the air; strong clavicles, large scapulæ; cubits, almost fixed in a state between pronation and supination, capable of very obscure rotation. Pectoral mammæ, in some genera pubial papillæ; teeth of three kinds, more or less distinct; in number, form, and duration anomalous.

This type includes about forty genera, the common index, *viz.*, the volant form, obvious; the subordinate characters less evident, and occasionally obscure. Three races or districts may be traced, the third of which at least is divisible; further discoveries will probably lead us to correct all present distributions.

1*st Race*, PLEURONYCTERATES, Vaulters, or feline bats (Lemur volans, Galeopithecus). Digits of all the extremities of nearly equal lengths; pannicle a parachute attached to the sides and legs, not predominating on the anterior members; all the digits furnished with nails, those of the fore-paws very crooked. Incisors $\frac{4}{6}$, short and jagged; canines $\frac{2}{2}$, ambiguous; molars $\frac{10}{10}$ rough; cæcum large. This race, or district? contains but a single kind.

Kind, Pleuropteridæ, and of this kind but a single genus as yet is known; formerly esteemed a Lemur, and called L. Volans, since Galeopithecus, or feline ape, both very inappropriate terms, as it hath as little in common with the manupeda, if not less, than its congeners, and neither in teeth nor intestines does it associate with beasts of prey, even so much as the other bats. Pleuropterus or Dermopterus would either form a more fitting name.

2*d Race*, MEGANYCTERATES, Roussettes, or Greater Bats (Harpies). Anterior digits lengthened and wing-like, fore-index generally with a nail, thumbs separate, but not opposable; incisors, canines, and molars truly cutting, tearing, and grinding teeth, the latter truncated, rough, but not pointed, a longitudinal groove traversing the whole; no cæcum, frugivorous: interfemoral membrane but little developed. External ear always single. The genera may be referred to one kind.

Kind, Pteropusidæ, although it is probable they will hereafter, or would even now, admit of subdivision.

3*d Race*, NYCTERATES. True bats, (Rere-mouse, or Flitter-mouse). Anterior members wing-like, incisors variable, jagged; molars pointed: insectivorous. External ear furnished with an auricle, more or less complex.

Kinds, Noctilionidæ, Rere-bats; fore-index 2 osseous phalanges, middle digit. 3.

Vespertilionidæ. flitter-bats, fore-index 1 osseous phalanx, other digits only 2.

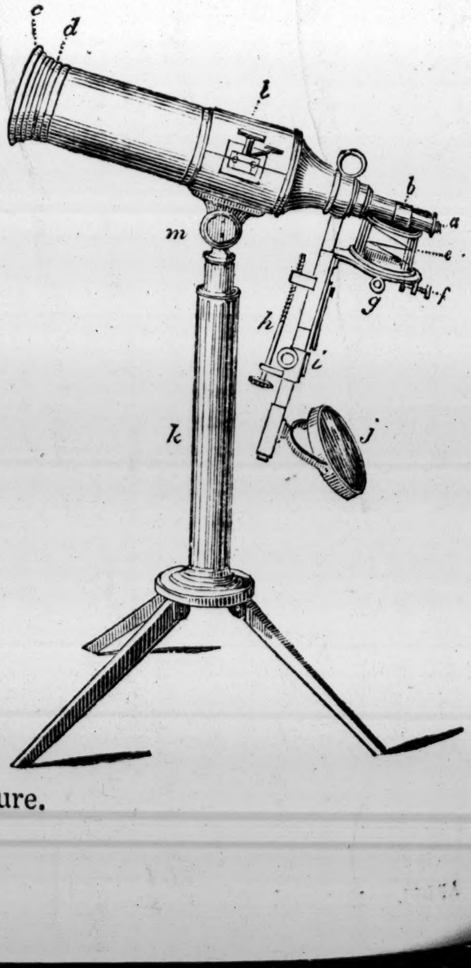
Type.	*Race.*	*Kind.*	*Genus.*	*Species.*	
ALIPEDA, ALIPEDS, WING-FOOTED BEASTS.	Pleuronycterates, or Parachute Bats,	Pleuropteridæ, Vaulters,	Pleuropterus, or Galeopithecus	Rufus,	Red Vaulter.
				Variegatus,	Spotted.
				Ternatensis,	Ternate.
	Meganycterates, or Great bats,	Pteropusidæ, Roussettes,	Pteropus	Vulgaris,	Com. Roussette.
			——	Edulis,	Eatable.
			Cercopteropus ?	Ægyptiacus,	Egyptian.
			——	Amplexicaud,	Long-tailed.
			Cynopterus	Marginatus,	Ear-banded.
			Macroglossus	Cuvieri,	Kiodote.
			Tribonophorus	Desmarestii,	Mantled R.
			Cephalotes	Peronii,	Peron's.
			Harpyia ?	Pallassii,	Harpy.
	Nycterates, or True bats,	Noctilionidæ, or Rere bats,	Molossus	Ater,	Blk. Bull-dog b.
			——	Rufus,	Red.
			Cheiromeles	Torquatus,	Collared C.
			Nyctinomus	Plicatus,	Plaited Nyc.
			——	Tenuis,	Lowo churut.
			Noctilio	Leporinus,	Hare-lipped N.
			——	Albiventer,	White-bellied.
			Phyllostoma	Hastatum,	Javelin bat.
			——	Perspicillatum,	Spectacle.
			——	Spectrum,	Spectre.
			Vampyrus	Cirrhosus,	Bearded Vampyre
			Istiophorus ?	Soricinus,	Shaven.
			Tonatia ?	Bidens,	Two-toothed.
			Glossophaga	Caudifer,	Tailed Gloss.
			——	Ecaudata,	Tail-less.
			Mormoops	Blainvillii,	Blainville's M.
			Madæteus	Lewisii.	Lewis's M.
		Vespertilionidæ, or Flitter bats,	Megaderma	Spasma,	Spasm bat.
			——	Trifolium,	Trefoil bat.
			Rhinolophus	Ferrum equinum,	Horseshoe.
			——	Tridens,	Trident.
			——	Speoris,	Pit-nosed.
			Nycteris	Geoffroyi,	Geoffroy's bat.
			——	Hispidus,	Hairy Nyc.
			Rhinopoma	Microphylla,	Small-leaved R.
			Taphozus	Longimanus,	Long-armed T.
			——	Perforatus,	Perforated T.
			Myopterus	Daubentonii,	Daubenton's.
			Celæno	Brookesiana,	Brookes's.
			Aëllo	Cuvieri,	Cuvier's.
			Scotophilus	Kuhlii,	Kuhl's.
			Artibæus	Jamaicensis,	Jamaica.
			Monophyllus	Redmani,	Redman's.
			Diphylla	Ecaudata,	Tail-less D.
			Dysopes	Mops,	Cuvier's D.
			Nyctophilus	Geoffroyi,	Geoffroy's N.
			Thyroptera	Tricolor,	Three-coloured.
			Proboscidea	Rivalis,	River Prob.
			——	Saxatilis,	Rock.
			Furia	Horrens,	Fury.
			Vespertilio,	Murinus,	Common Bat.
			——	Serotinus,	Serotine.
			——	Nasutus,	Great Serot.
			——	Pictus,	Striped.
			——	Temminckii,	Temminck's.
			——	Hardwickii,	Hardwick's.
			Plecotus,	Auritus,	Long-eared.
			——	Barbastellus,	Barbastelle.
			——	Velatus,	Veil-eared.
			Atalepha ?	Sicula,	Sicilian.

An Account of the latest Modifications in the Structure of the Amician Catadioptric Microscope, with full and particular directions for managing and observing with it. By J. Cuthbert. Communicated by the Author.

Dr. Goring, in his account of the improvements which have been made in England on the Reflecting Microscope of Professor Amici, has given directions for using the instrument, but such as I find by experience to be inadequate to enable observers, not greatly habituated to the use of microscopes, to manage it *in an effective manner*. The following description and instructions are intended, with the help of the wood engravings, to supply the place of a *vivá voce* lecture upon the instrument itself, and are adapted to the standard of the meanest capacity.

When the instrument is removed from the box, nothing more is required than to reverse the legs, and draw back the slide on the small tube containing the reflectors.

Fig. 1,



Shews the instrument ready for use.

(*a.*) The tube containing the reflectors.

(*b.*) The slide which covers the aperture, to preserve the reflectors when not in use. The tube can be unscrewed to substitute the other reflectors, which differ in magnifying power.

(*c.*) The eye-tube, which can be drawn out to apply others, which also vary the power.

(*d.*) Shews the milling of an internal tube, which may be partly drawn out, thereby lengthening the body, and thus giving a further increase of power, or intermediate powers between those produced by the eye-tubes.

(*e.*) The slider holder for supporting the objects; it may be detached from the stage at pleasure.

(*f, g.*) Two adjusting screws, by which the slider holder can be moved in cross directions for the purpose of bringing any part of the object to the centre of the field of view.

(*h.*) The screw by which the object is adjusted to the focus.

(*i.*) The milled head which clamps the screw apparatus to the bar of the instrument, after roughly adjusting the focus, by moving it up and down on the bar: the correct adjustment is effected by the screw *h.*

(*j.*) The illuminator, which has a concave and a plane surface, to be used at discretion.

(*k.*) The pillar, composed of two tubes; the internal one can be drawn out to raise the instrument, either for the convenience of the observer, or to regulate the light from the lamp, which should also have a similar adjustment.

(*l.*) The screw for tightening the cleft-socket, by which means the body of the instrument with its apparatus can be kept in any required position, the variety of motions given by means of the cleft socket *l*, the joint *m*, and the sliding pillar *k*, leaves nothing further to be desired, either in point of utility or convenience.

Fig. 2,

(*a.*) Shews the position of the small condensing lens placed on the neck of the body; it is capable of being moved in any direction, to condense the rays from a lamp, &c., to illuminate an opaque object.

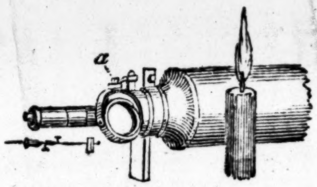


Fig. 3,

Shews the instrument in its best position, when direct daylight is intercepted for shewing transparent objects without the illuminator; and *Fig.* 4 for direct lamplight, &c.

Fig. 3.

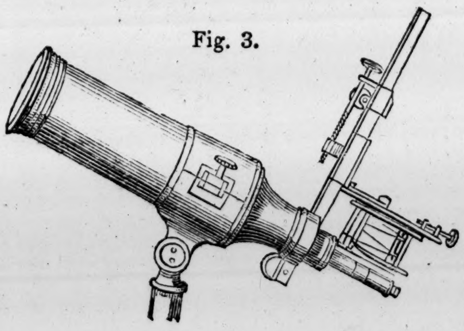


Fig. 4.

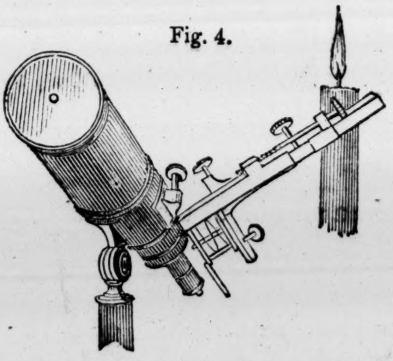


Fig. 5.

The universal forceps, which fixes on in place of the stage (as represented at Figs. 6 and 12); it will be found very convenient, affording great facility of presenting all parts of the object under examination to the action of the reflectors.

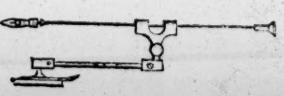
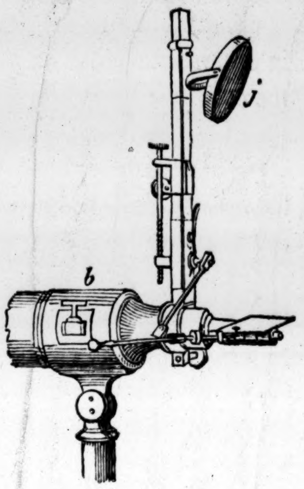


Fig. 6.



Shews the body turned round vertically in its spring socket *b*, until the aperture in the tube containing the reflectors is directly upwards and beneath the mirror, for the purpose of viewing crystallization of salts, &c., which are placed on a piece of talc, held in the forceps over the aperture, by which arrangement the reflectors are preserved from being injured by the steam of the salt entering the aperture. The light is then to be directed or condensed by the illuminator *j*, through the fluid.

Fig. 7,

Is a piece to connect the forceps with the traversing work of the stage, used in lieu of the slider holder, *e*, Fig. 1, (which is liberated by being turned a quarter round,) the piece is then capable of being moved by the two adjusting screws *f* and *g*. In some cases it may be found convenient to remove the stage wholly from the socket of the adjusting screw apparatus, and apply the forceps in its place. In that case the connecting piece, Fig. 7, is not applied.

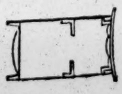
Fig. 8,

Is a moveable object-carrier, which slips between the bars of the sliderholder, *e*, Fig. 1, and is used for holding concave glasses, or a box to contain living aquatic objects, for animalculæ, &c.; also a black surface, for giving a ground to opaque objects.

Fig. 9,

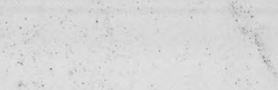
Is the live, or aquatic box, the glasses of which are capable of being placed at various distances, to suit the particular kind of object.

Fig. 10,



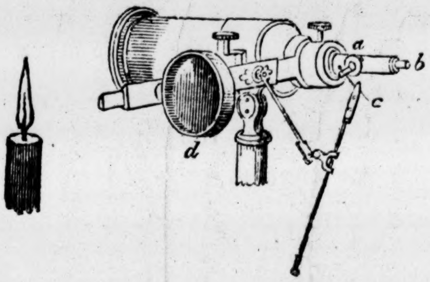
Is a section of the eye-tube, shewing the position of the lenses.

Fig. 11,



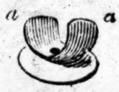
Is a section of the tube and reflectors, shewing their position in sitû, *a*, the aperture through which the rays pass from the object to the plane reflector *b*, which is placed at an angle of forty-five degrees, and directs the rays towards the concave reflector *c*, from which they are transmitted through the body and eye-tube to the eye of the observer.

Fig. 12,



Is an oblique view, shewing the position of the instrument for examining opaque objects; *a,* the silver cup with its aperture centrical to that of the tube *b*; *c,* the object to be viewed placed directly opposite the aperture of the said tube, so as to prevent any side light from entering; *d,* is the large condensing lens which slides upon the bar, instead of the double reflector, and is to be placed about four inches from the silver cup *a.* The candle, or lamp, must also be about the same distance from the lens.

Fig. 13,

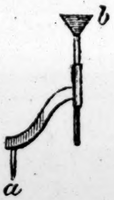


Is the silver cup, shewing the spring clamps *a, a,* which attach it to the tube of the reflectors.

Fig. 14,

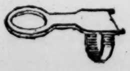
Shews one of the cork opaque object-holders placed on a pin having an object stuck on each side. They are preserved in a box with a cork bottom to fix them into. The small-sized holders are for the high powers.

Fig. 15,



Is a black ground to be used when a small opaque object is held in the forceps *unattached to a cork holder*, and requiring to be turned round to exhibit its different parts: it is connected to the socket of the tightening screw, Fig. 1, *i,* by the pin *a*; the black disk *b,* also serves to prevent false light from entering the aperture. The wire which holds it can be adjusted to the requisite distance by sliding it up and down in the arm which supports it. Disks of different sizes are adapted to the different illuminating cups.

Fig. 16,



Is an arm to carry single lenses; it slips on the neck of the body in the place of the tube of reflectors, thereby making a most convenient single microscope, if required.

The most convenient method of proceeding to examine objects is as follows:—After setting up the instrument, and drawing back the slide on the tube of reflectors, turn the illuminator towards the light, and at such an angle, that the rays may be thrown up to the small aperture of the tube, containing the reflectors, when the field of view is perfectly illuminated, an even, round, and bright disk of light will be seen in the body of the instrument, or the illuminator may be removed, and the body of the instrument turned round, to procure the simple and unreflected light, which, in most cases, will be found the best method of procedure. (See *Figs.* 3 and 4.) The slider containing the object to be examined may then be introduced between the bars and spring plate of the slider-holders, remembering to place that side of the slide on which the brass ring is not seen, next to the tube of reflectors. The object may then be adjusted to the focus in the following manner:—

First slack the screw (*i*, *Fig.* 1), then slide the whole of the screw apparatus up or down, till the object is nearly in the focus; then tighten the screw (*i*) and perfect the adjustment, by turning the milled head of the screw (*h*). It is proper to remark, that, as the brightness and ease of vision must necessarily decrease with the increase of magnifying power, it should be so arranged as to suit the particular kind of object under examination, using no more amplification than is absolutely necessary. A low power is best suited to make out the general character of an object in the first instance; it may then be increased for investigating its more minute parts.

The low powers may be used for opaque objects, without the condensing lens, *if the daylight is bright*, or by merely bringing the lamp towards the side of the tube, containing the reflectors. When the higher powers are used, the small condensing lens, as shewn in *Fig.* 2, must be made use of, placing the light the same distance from the lens as the lens is from the object, viz., about an inch and a half.

The most powerful mode of illuminating for high powers is obtained by the use of the condensing cups and large lens, the arrangement of which is shown by *Fig.* 12. This method shows objects in a brilliant light, with scarcely any shadow, and answers well for the generality of opaque objects.

Some require oblique lights to produce a shadow, such as the fine lines on the scales of the diamond beetle, which can scarcely be seen by a direct light, and, in such cases, the small condensing lens must be used as described (*Fig.* 2). The focus of the reflectors may be made to coincide exactly with that of the condensing cups by a small elongation of the tube, (*d*, *Fig.* 1). Care should be taken that the tubes containing the reflectors are screwed perfectly home, so that the aperture may be truly opposite the slider-holder, and in the line of the bar; a rase is cut round the tube of reflectors, as a rough guide for bringing the object under the aperture. The shortest tube of reflectors is the most powerful of the set: the focus of its concave reflector is $\frac{3}{10}$ of an inch, its diameter $\frac{2}{10}$ full, and the object requires to be placed about $\frac{1}{20}$ of an inch from the side of the tube.

The second pair of reflectors is $\frac{6}{1}$ of an inch focus, and aperture $\frac{3}{10}$; its focal point is about $\frac{1}{10}$ of an inch from the side of the tube.

The third pair of reflectors is one inch focus, and full $\frac{3}{10}$ of an inch aperture; the focal point about $\frac{1}{5}$ of an inch from the side of the tube. The longest tube of reflectors is 2 inches focus, and $\frac{1}{2}$ an inch aperture; the focal point is half an inch from the side of the tube: from its small power, it is that best adapted for the larger objects. The instrument is furnished with three eye-tubes, and, like the tubes of reflectors, the shortest magnifies the most. Either of the eye-tubes can be used with the different tubes of reflectors, *observing that it is always best to gain the power by the reflectors.* Use, therefore, short reflectors and long eye-tubes.

When the greater powers are wanted, of course the short eye-tubes are to be used. By lengthening the body of the instrument, a great increase of power may be obtained.

In some instances an inconvenience occurs from this mode of gaining power, *as the focus is brought nearer to the side of the tube;* of course the light is not then so easily applied to opaque objects.

The powers given by the combinations are nearly as follows. The long tube of reflectors, with the combinations of the *three eye-tubes*, magnifies from 5.00 to 5.000 times *in superficies;* the next in length, from 3.000 to 50.000; the next tube from 8.000 to 160.000; the short tube up to 320.000; the elongation of the tube produces a further increase up to 1,000,000 times. The most generally useful reflector will be found to be the shortest but one. The shortest one is only requisite to define the most difficult class of *lined objects.*

Particular care should be taken to keep the tubes of reflectors closed when not in use: the metals will not require any cleaning, and should not be removed from their tubes. The glasses of the eye-tubes require to be most attended to, as dust or finger-stains upon them very much lessen the brightness and distinctness of vision. When the eye-tubes are cleaned, only one glass should be removed at a time, and wiped with a soft piece of leather, *and then replaced, before another is taken out,* to prevent the misplacing of them, as such a simple mistake would render the instrument nearly useless.

The talc between which the objects are placed being of a soft nature, care should be taken not to scratch or soil it with the fingers. Dust is best removed by a camel hair pencil, and stains by a soft piece of leather. It often occurs in cold weather, that, when the instrument is used, it is considerably below the temperature of the room or observer, and, consequently, a condensation of vapour takes place on the glasses of the eye-tubes, which prevents distinct vision, and though wiped off, will still continue to condense from the eye of the observer. Till the instrument has acquired sufficient warmth to prevent such impediments, it is requisite to place it for a short time before the fire.

The most delicate test objects are the lines on the feathers of butterflies or moths' wings, of which there are many gradations; some easily demonstrated, and others more difficult to be seen, and then only with the most powerful reflectors, and seen to the best advantage by the simple and uncondensed light of the lamp. The light must be so arranged that the rays will pass through them in an oblique direction; also the position of the object must be attended to, for in some positions not a line will be seen, when a little variation of the light may render them perfectly distinct. The hair of a mouse is a very good test object: it is best seen by daylight; the most difficult parts of which are longitudinal lines in the transparent part of the hair, which require high powers. The hair of the bat and seal are also fine tests. The lines on the scales of the diamond beetle, &c., are excellent opaque proof objects. The feet of flies are likewise very interesting.

It does not become me as a maker of these instruments to say more of them than that they have been compared with the best microscopes now extant, and have given the greatest satisfaction; and that it is the opinion of most observers that the vision from truly-executed specula produces a degree of delicate distinctness which no refracting microscope can rival.

5, *Purbeck-Place, Lambeth.*

Remarks on the Discussion between Mr. Ivory *and* Mr. Meikle, *on the Temperature and Condensation of Air.*

Sir,

If you consider the following remarks worthy of a place in the *Quarterly Journal of Science*, their insertion will oblige

Your obedient Servant,

A Civil Engineer.

The laws of heat as connected with air, being a subject of considerable importance as a branch of natural philosophy, as well as in its various applications to useful science, I have been induced to offer a few remarks on the papers lately published on this subject by Mr. Ivory, in the *Philosophical Magazine* and *Annals of Philosophy*, and by Mr. Meikle in the *Quarterly Journal of Science*.

From the high character of Mr. Ivory as a mathematician, I should have been almost inclined to consider any such weighty charges of error brought against him in a manner so unceremonious as those by Mr. Meikle have been, as undeserving of attention, had not two papers of considerable length appeared from Mr. Ivory in his defence; and as the peculiar manner of his replies seemed to betray strong symptoms that there might possibly exist some grounds for the accusations, I now find that, after an attentive and impartial examination of both sides of the question, my suspicions have been completely realized.

The two principal points of Mr. Ivory's doctrine which Mr. Meikle impeaches are the following:—

"1st. That the heat extricated from air when it undergoes a given condensation is equal to $\frac{3}{8}$ of the diminution of temperature required to produce the same condensation, the pressure being constant.

"2d. That while the ratio of the specific heat under a constant pressure to that under a constant volume is invariable, equal quantities of absolute heat produce equal increments of volume."

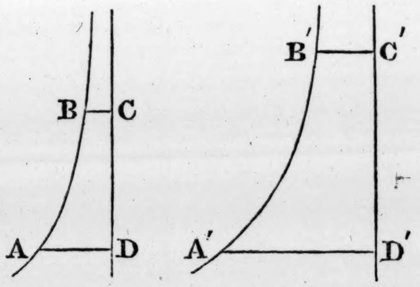
As to the first of the above propositions, the point to be settled is briefly this;—whether Mr. Meikle has correctly expressed it by the formula

$$i\frac{8}{3}\left(448+\tau\right)\times\frac{\varrho-\varrho'}{},$$

for if so, the whole train of absurd consequences pointed out by him must inevitably follow. The manner in which it is deduced, given by Mr. Meikle in his last reply, being extremely simple, and had it contained any error, Mr. Ivory could not have failed to perceive it; and instead of writing a reply of six pages in length, consisting, as Mr. M. observes, of little else than a repetition of what he had formerly published, why did he not at once point out the error, which he might have done in little more than as many lines;—the *trick*, of which he complains so bitterly of having been played upon him, would thence have been exposed, and, consequently, the whole of the argument of his opponent would have fallen to the ground. In short, Mr. Ivory's complete silence on this head affords the strongest presumptive evidence that he was unable to discover any such error to exist, otherwise he would never have shrunk from this his only consistent mode of defence.

With regard to the second proposition, Mr. Meikle seems to admit the first part of it, respecting the inconstancy of the ratio, but denies the second as inconsistent with it, and certainly the demonstration which he has given to shew this is both ingenious and conclusive. It only remains to inquire into the grounds which Mr. Ivory appears to have had in support of his opinion. Accordingly, on looking over his papers on the subject, what appears to bear most on this point is the following passage in the No. of the *Philosophical Magazine* for February, 1827.

"If we compare two thermometers, one of air and the other of mercury, which have their scales adjusted to the fixed points at which water freezes and boils, and find that their indications agree for a long range of temperature, we *must* infer that the supposed principle is true in nature for the whole of the interval." That such a deduction as this should have been made by a profound mathematician, which Mr. Ivory must undoubtedly be allowed to be, seems in no small degree surprising, as nothing appears more clear than that the only inference which can be made from the above acknowledged fact is, that the law of expansion, or, which is the same thing, the variation of capacity for heat, is the same in both bodies, but it affords no means whatever in determining the nature of that law. This may be easily shown by a geometrical diagram in a manner similar to that in which Mr. Meikle has demonstrated the law of variation itself.



Let A B and A′B′ be any two similar curves, having asymptotes C D, C′ D′; and let the ordinates A D, A′ D′ represent the respective capacities of the two bodies for heat, the abscissas CD, C′D′ the corresponding expansions for any given range of temperature: the points CD, for example, may be considered as the freezing and boiling points of water. Now it is obvious that if the ordinate A D always express the capacity at the point D of temperature, the space A B C D will be proportional to the quantity of heat employed in raising the temperature from D to C; and if C D, C′ D′ be each divided into the same number of equal parts or degrees, and continued indefinitely above and below the points C and D, it is also evident that while the law of variation of specific heat in both substances continues the same, the points C, C′ will always correspond with the same degree in the asymptotes, when each body is exposed to the same intensity of heat. This, it is presumed, will be readily admitted by any mathematical reader, without a formal demonstration being given.

The foregoing data, however, furnish no means of determining the equation of the curve AB; but when the ratio of the specific heat under a constant pressure to that under a constant volume, as was shown by the experiment of MM. Clement and Desormes, is always the same, it is thence known to be that of an hyperbole, as Mr. Meikle's demonstration very satisfactorily proves.

Although I can by no means agree with Mr. Meikle in conceiving that Mr. Ivory has written his papers on this subject in such a manner as intentionally to bewilder his readers, nor can I, on the other hand, consider the former gentleman in the light of a juggler, with his cups and balls, making a sport of science at the expense of his opponent, yet I certainly think it a matter of regret that a man of Mr. Ivory's acknowledged reputation should continue to persevere in the assertion of doctrines so utterly untenable, instead of candidly confessing what the tribunal of the public must eventually condemn.

The present case, however, is not a solitary instance of an eminent mathematician being found out committing palpable errors in reasoning on physical subjects. Among others, the great John Bernouilli, though possessed of penetration and sagacity almost superhuman, into what inconsistent absurdities was he not sometimes led in the investigation of physical problems!

I have been the more urgent in endeavouring to draw the attention of scientific men to the above subject, as I find that Mr. Ivory's rule has been copied with implicit confidence into a very useful little tract on the steam-engine, and extensively circulated among the practical men throughout the great mining district of Cornwall. The same has also been copied in the *Quarterly Journal*, and it is to be feared that it has found its way into many other works. Any error in a practical rule emanating from so high an authority, ought to be a matter of no small moment with the public, especially when it has been adopted as standard authority by practical men who, though they may be well skilled in applying it, can seldom be supposed competent to judge of the accuracy of the principles from which it is deduced.

Notes on Medical Music. By William Wadd, Esq., F.L.S.

> Music exalts each joy, allays each grief,
> *Expels disease*, and softens every pain,
> Subdues the rage of poison, and the plague.
> And hence the wise of ancient days adored
> One power of physic—melody—and song.

Such is Armstrong's account of the medicinal powers of music, from which we may understand that, in the earlier periods of the world, the effects of the specifics in a modern apothecary's shop, stimulant and sedative, were produced by some spirit-stirring air upon " the noble pipe, or nobler lyre." However this might have been in the days of Orpheus and Amphion, we should deem Drs. Warren and Maton worthy of castigation, if, to relieve the pains of cholera or gout, they produced their *cremonas* in the hope of effecting a cure by the *charms* of a medical duet. Indeed, if this could ever have been done, to the extent which poets pretend, the musical talents of medical men have been most strangely and unnaturally neglected. In the whole range of our medical acquaintances, we do not recollect half a dozen good musicians. Poor Pemberton was a delightful vocalist, yet lacked the aid ascribed to melody: St. Cecilia herself could not have effected his cure. Rowley, the redoubted and cogent anti-vacinator, was " music mad," yet not mad enough to dream of its being a specific. Sharp, the surgeon, Sir Charles Blicke's master, was a great amateur, but he never used music as a means of curing patients, only in attracting them. It was said that he "fiddled himself into practice, and fiddled Mr. Pott out of it;" certain it is Mr. Pott, not being a *flat*, did not choose to act in *concert* with *Sharp*, and made a quick movement to the westward. The only musical doctor we remember noticed in print, was John Langdon, whose epitaph is noticed by Ant. à Wood:—

Musicus et Medicus Langdon jacet hicce Johannes.

The Asclepia, in Greece, were feasts in honour of Æsculapius, chiefly consisting of music, and were occasionally called Μηγαλασκλατεια, or the great festivals of Æsculapius. It was also said of Chiron, that he would sometimes strike the shell-resounding phorminx of Mercury.

One of the most amusing stories in ancient history, of the successful and happy use of fine music, is told of Arion, who, when about to be thrown overboard by some mutinous sailors, begged leave to sing to his lute one funeral strain before his death. Having obtained leave, he stood upon the prow with his instrument, chanted with a loud voice his sweetest elegy, and then threw himself into the sea. A dolphin, as the story goes, charmed with his music, swam to him while floating on the waves, bore him on his back, and carried him safely to Cape Tænarus, in Sparta, from whence he went to Corinth. It would have been well for the mutineers if their taste for music had been as great as the dolphin's, for the history not only affords a grand instance of the power of music, but of retributive justice, as the sailors accidentally going to Corinth, paid the penalty of their evil intentions with their lives.

Kircher, in his *Musurgia*, makes a little digression from his subject to give a musical mode of catching fish, (piscis Phyphiæ), which ought to satisfy the incredulous. He says, "Ego dum an. 1638, 17 die Maii, Missanum transirem, totam procedendi rationem ab ispsis piscatoribus, venationi præsens, accepi, verba, quibus piscem, allectare solent, sunt sequentia.

Mamassu di paianu
Pallettu di pianu
Maiassu stignela
Pallettu di paenu palè
La stagnela
Mancata stignela
Pro nastu vardu pressu da visu et da terra."

Athanasius Kircher, or Father Kircher, as he was called, in his elaborate and learned work, *Musurgiæ Universalis*, treats of the medical powers of music: "Musurgico-Iatrica." "Iatricam sive medicinam musurgicam eam vocamus, quæ in curandis animæ corporis morbos effectus prorsus exitos, et prodigiosus præstat."

He enters most minutely into the causes, the modes, and manner in which music, according to the notions of the philosophers, astrologers, and alchemists, produces its medical curative effects.

The earliest medical application of music we know of was in curing a pestilence, which afflicted the Greek army before the walls of Troy. Thaletas of Crete applied the same remedy, with equal success, to the suffering Lacedemonians; and the ancient writers celebrate the powers of harmony, in resisting, and even curing infectious malignities, in most animated strains.

Music the fiercest grief can charm,
And Fate's severest rage disarm.
Music can soften *pain* to *ease*,
And make despair and madness *please*.

M. Burrette (Vol. XV. *Mem. Acad. de Belles Lettres*) mentions many diseases cured by music, and cites many Greek and Roman authors to the fact. Marianus Capellus assures us, that fevers may be cured by appropriate songs. Asclepiades employed the sound of a trumpet as a remedy; Democritus, that of a flute; David calmed the fury of Saul by the tone of his harp. Athenæus asserts, that the flute, played after the Phrygian mode, will cure sciatica. Le Dorien, or Aulus Gellius, on the contrary, recommends plaintive music. Cœlius

Aurelianus even determines the length to which this species of remedy should be carried; "*quos cum saltum sumerent palpitando, discusso dolore, mitescerunt.*"

To understand this, it may be proper to state, that the ancients had four modes, or kinds of music, thus described by a modern French physician: "Le mode Phrygien excitait le courage et la fureur; le Lydien, la tristesse, les plaintes, les regrets; le Eolien, la tendresse et l'amour; le Dorien, la piété, et le respect pour les dieux."

And here, to show the early connexion between music and medicine, we must quote an anecdote of Galen, which, as the French doctor justly observes, proves that the ancients, in their different modes of music, exercised "*un pouvoir surprenant.*" "Damon, musicien de Milet, en jouant de la flûte sur le mode *Phrygien*, avait excité à la fureur des jeunes gens pris de vin; (in this state of 'les jeunes gens,' the doctor is sent for,) Galien lui ordonna de changer de mode et de jouer sur *le Dorien: aussitôt ces jeunes gens passèrent, de l'état le plus violent au calme le plus parfait!*" Cours Elémentaire D'Hygiène, par L. Rostan.

It is written that, "when Saul was troubled with an evil spirit, David took an harp, and played with his hand: so Saul was refreshed and was well, and the evil spirit departed from him." Every diligent observer of that sad disorder, hypochondriasis, must have remarked the animation and relief sometimes afforded, when

> Music her soft assuasive voice applies.

The use of music in curing the effects of the bite of the tarantula, as practised in Apulia, is well known; but it has been surmised, that the perpetual dancing, recommended as an accompaniment, might have had something to do with the unaccountable expulsion of the poison. Baglivi, who treats largely on this subject, has a chapter, "Chorea et Musica sunt præcipuum hujus Veneni antidotum. Ubi obter modus saltandi, Apuliæ familiaritur describitur."

Shadwell, the poet, in his comedy of the Virtuosi, ironically alludes to this musical remedy for poison. Sir Nicholas, the virtuoso, is made sarcastically to philosophize on it. He says, 'I have observed the *Tarantula* does infinitely delight in music, which is the reason of its poison being drawn out by it. There's your phenomena of sympathy!"

Long.—"Does your *Tarantula* delight so in music?"

Sir Nic.—"Oh, extravagantly. There are three sorts, black, grey, and red, that delight in three several sorts and modes of music."

But who is to heed the sneers of the poet, when we have Dr. Mead, as well as Baglivi, explaining, on philosophical principles, the manner in which music operates in allaying the tumult in the constitution occasioned by the poison, and moreover very strongly intimating that the practice originated with Pythagoras. Perhaps it may be thought, by less learned persons, that the musical specific for the Tarantula's bite had as fair claims to a place in the Pharmacopœia, as the "Pulvis Antilisus," or the equally potent "Pulvis Epilepticus Niger!"*

The French physicians seem to entertain much higher notions of the power of music over the animal economy than we do. Desault, a physician at Bourdeaux, goes so far as to believe in its efficacy for curing mental derangement, hypochondriasis, epilepsy, and even hydrophobia!

Another celebrated French writer (Rostan) introduces a chapter, entitled, "*Effet du son, comparé sur l'ouïe, et sur l'organisme entier;*" in which he shows how the functions of life are to be affected by different modifications of music—how the circulation, respiration, and digestion, are to be accelerated or retarded. "See," says he, "how the soldier sustains the fatigues of a march by the sound of the fife and drum! And how a sickly girl, who cannot stir three paces in the day, will dance a whole night by the aid of music, which he further illustrates by the case of a young lady, whom he ordered to attend some baths at a distance from her house, to which she was to go on foot. The fatigue, however, was so great as to prevent her taking any exercise the rest of the day. It happened, however, one morning, that a regiment, preceded by a band of music, marched along the road as she was returning, and she found herself at home without being aware of it, and moreover, without her usual lassitude. She was too sensible a young lady not to profit by this event, and as the regiment passed every day, at the same hour, it was very easy to repeat the experiment, which was invariably accompanied with the same success.

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* Mead declared, that in the experience of thirty years, he had never known the Pulvis Antilisus fail in curing hydrophobia, and that he witnessed its success in 500 cases! *Dr. Diedrich Wessel Linden,* the parent of the "Pulvis Epilepticus Niger," was equally successful with his specific; when, by great labour and expense, he had made it potent and perfect by the addition of a bone or two of pig's head, to which he gave the grandiloquent term of "Ossicula Epileptica," he thought it important enough to be described by a copper-plate and a pamphlet! Like Columbus, he made this great discovery by traversing the seas, and with a magnanimity worthy of the *discovery* and the *discoverer,* he gives up his secret, "thinking it would *be a crime to conceal so valuable a remedy any longer from the world.*"

Shakspeare seems fully to appreciate the various virtues of music: its stimulating and sedative effects; the brisk "ear-piercing fife," and the soothing note, that "comes o'er the ear like the sweet south!" (*Twelfth Night.*) Of its narcotic powers, he exclaims,—

> "Most heavenly music!
> It nips me into listening, and thick slumber
> Hangs on mine eyes."

Again, in *The Midsummer's Night Dream*,—

> "Music call, and strike more dead
> Than common sleep, of all these five the sense."

So Tamora, in *Titus Andronicus*, says,—

> "Be unto us, as is a nurse's song
> Of *Lullaby* to bring her babe to sleep."

A learned commentator gives us what he facetiously calls a lullaby note on this.

> "The verb to *lull*, means to sing gently, and it is connected with the Greek λαλεω, loquor, or λαλα, the sound made by the beach of the sea. The Roman nurses used the word *lalla*, to quiet their children, and they feigned a deity called *Lallus*, whom they invoked on that occasion; the lullaby, or tune itself was called by the same name." (*Douce.*)

Lullaby is supposed a contraction for *Lull-a-baby*. The Welsh are celebrated for their Lullaby songs, and a good Welsh nurse, with a pleasing voice, has been sometimes found more soporific in the nursery, than the midwife's anodyne. The contrary effects of Swift's song, "Here we go up, up, up," and the smile-provoking melody of "Hey diddle, diddle," *cum multis aliis*, are too well known to be enumerated or disputed. "The Good Nurse" gives us a chapter on the advantage of employing music in certain stages of protracted illness.

Ame (soul) has been made a term in music by the French, who have their "*Airs Tendres*," as well as the Swiss. The effect of soft music is to produce pleasure or pain, according to the state of the hearer. Thus, while a musician has been known to be *cured* by a concert in his chamber, the celebrated sentimental air of the "*Ranz des Vaches*" has also been known to have the opposite effect of *killing* a Swiss. Indeed, the extraordinary effect produced by it upon Swiss troops has caused it to be forbidden, under *pain of death*, to be played to them.

Polybius ascribes the cruelty of a certain people to their disuse of music, and therefore gravely, and earnestly, advises the rest of the *Arcadians* to keep up their musical exercises, lest they should turn *barbarians*. It is said, that Augustus was cruel and savage, before he was civilized by the harmonious numbers of Virgil, and Horace, who, Orpheus-like, by their songs, transformed a brute into a man. Timotheus, we read, swayed the passions of the Great Alexander just as he pleased; Phémius disarmed Ulysses; and the bloody Amurat IV. was affected even to tears by the performances "d'un joueur de psalterion."

Dr. Wallis has endeavoured to account for the surprising effects ascribed to ancient music, by the novelty of the art, and the hyperboles of the ancient poets. He conceives that modern music, *cæteris paribus*, would produce equal, if not greater effects, and adduces the power of a modern musician over Erric, king of Denmark*. Dr. South has founded his poem of "Musica Incantans," on the power of music over the mind. Derham, in his "Physico-Theology," has some curious observations to the same point. We must recollect also, that ancient music was of a larger extent than at present: poetry and dancing were accounted parts of it. Verse itself, with good cadence, and good language, will strangely work upon the fancy. There was a time when it was believed that life might be taken way by metrical charms. As superstition grew weaker, these charms were imagined only to have power on irrational animals: in our days, there are Irishmen who fancy they can rhyme a rat to death.

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* The learned Gesner, in his notes on Quinctilian, confirms this opinion, for he says that Bach (Sebastian) would effect more than twenty Orpheuses, and as many Arions. "Maximus alioquin antiquitatis fautor, multos unum Orpheas et viginti Arionas complexum Bachium meum, et si quis illi similis sit forte, arbitror." Nor can we doubt this, when we read of the enthusiasm with which Handel was followed. It is related that he and Bach once met accidentally at Saltzberg. On their going together to the cathedral, they found it so full that they could scarcely get into the organ-loft. The church was crowded to suf-

The effect produced by musical voices is singularly evinced by the power of the missionaries over barbarians. It is said that St. Bernard preached "la Croissade aux paysans; et l'on sait de quel fureur ces bons gens étaiént agités, à ces sermons dont ils n'entendaient pas un seul mot." Boerhaave tells us, that one of the greatest orators of antiquity, Tiberius Gracchus, when animated, used to cry out like an old woman; to avoid which, he had a servant, who, at these periods, sounded a pipe, by way of hint, as well as to pitch the tone, so sensible was he of the importance of a well-regulated voice.

The writers on music draw their extensive divisions into *mundane*, *human*, and *instrumental*, from an old black letter treatise by Boethius, 1499; which though so purely theoretic, as to be of no use in practice, was for a long time held up with great parade, and made the chief study, for those who took musical degrees in our universities. Modern music, is better adapted to please the ear, than to excite the passions. Whether it be the object of our Royal Academy for music to recover the three genera of the Greeks, and all the miraculous powers of ancient music, we know not; but in this they would only follow the example of John Anthony Baif, an enthusiastic musician, who, in the reign of Charles IX., established an academy near Paris, (vocibus, fidibus, et festulis constaret) with that laudable intention, and the undertaking was honoured

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focation, and those who could not gain admission to the interior of the building, procured ladders, and placed them at the windows, in order to gratify their ears by those passages, which the full organ would convey through all impediments.

with the patronage of Charles IX. and Henry III. He succeeded better in poetry, however, than in music.

It is a very useful thing, doubtless, to hear music now and then, when we are in good health: but, says Monsieur Rostan, "pour les personnes nées avec beaucoup de sensibilité, c'est un *besoin aussi naturel, que celui de prendre des aliments!*" and, therefore, he considers that "*la musique*, doit entrer dans un plan bien fait d'hygiène!"

The musician who soothes our sorrows, and diverts our mind from its cares during health, renders his memory dear to the refined part of mankind, in every civilized nation; but, according to the French philosophy, the physician who adds the musician's art to his own, merits a most conspicuous and honourable place in the Temple of Fame.

Perhaps the grave reader may not admire these medical *fancies*, and consider them out of *time* and *tune*;—he may despise the "charm of the lyre," and the "declamation des jambes;"—he may scorn the skill of Clementi, and hold in contempt the art of Vestris:—we will not say, with Shakspeare, that such a man is "fit for treasons, stratagems and spoils," but that he is not fit for a good doctor;—not fit to use that species of medico-musical instrument, the Stehescope. A man whose ear is not attuned to the "concord of sweet sounds," will never be believed to possess a ear so "critically nice," as to discover, by sound, whether a disease of the heart be in the left ventricle, or the right, or to distinguish either, or both, from aneurism of the aorta! The best advice we can give to such an unharmonious person is, to follow Mr. Shandy's example, and study the great Trismegistus, who will inform him,—

> Musica nihil aliud est, quam omnium ordinem scire.
>
> *Trismegistus in Asclepio.*

Letter from DR. MAC CULLOCH *to the* Editor, *respecting some Miscellaneous Inquiries.*

IN turning over my note-book, I perceive, among a variety of miscellaneous matter, of which many are subjects for distinct papers, some facts and remarks which I am tempted to transcribe for you in a letter, as being too short for any other purpose; while I am unwilling that they should perish, as so much of what is entered under this form does every day. In this instance, where they refer to statements in your own Journal, I am sorry to say that I can neither point out the writer nor the number; having neglected to enter either when the remarks on the particular passages were made.

It is observed somewhere there, that no analysis of the produce of gunpowder, after combustion, has been given by chemists, and that the generated gases remain unknown. If the author who has committed this oversight will take the trouble to refer to the Supplement of the *Encyclopædia Britannica,* he will find the analysis in question, both as it concerns the gaseous and the solid produce. I need not, therefore, transcribe it here, to occupy your pages for no purpose. At the same time, I would gladly see that analysis repeated now, by abler hands, and by the more accurate means of analysing mixed gases which the chemistry of the present day furnishes. It is thirty years since it was performed, and with no very refined apparatus; so that it is probable that the relative proportions are not as accurate as they might be rendered. I will not even say, now, that it is entirely correct in the mere substances; but they who laboured at that distant date, and undertook such an analysis also, as one of their first problems, will not be very severe on the errors that they may discover.

In one of your recent numbers there is also recorded the Discovery, by an Italian Chemist, of a concrete adipose substance to be obtained from Essential Oils. That discovery was published by myself, nearly twenty years ago, in Tilloch's Journal, though I have not the series at hand, so as to point out the year or number. Of many experiments which I afterwards made on it, there are none in addition to those published (if I now recollect right, in two papers) that are of much importance; but some further remarks made on some of the oils may be worth mentioning, as they lead to an important conclusion as to the essential oils in general.

In this paper, I observed that the formation of this substance, which bears a nearer resemblance to spermaceti than adipocire, depended on age; since it is never found in new oils. That it is the consequence of the action of oxygen, is probable, if not accurately proved; and it increases with the age of the oil. In some specimens of the ottar of rose, which I know to be at least thirty years old, the whole has become a nearly solid mass, from the formation of this substance. By extracting from it, with cold alcohol, such of the liquid oil as remained, it did not even divide into parts, but remained a somewhat porous yet continuous substance. But as to this particular essential oil, there is an observation of some practical value that deserves record. The spermaceti (since I do not choose to coin a name) is inodorous; and it appears that during this change the odoriferous principle, be that what it may, is destroyed. This is proved by these specimens; for I did not procure from a drachm of this ancient oil more of the odoriferous, alcoholic solution, called essence of rose, than a few grains of the fresh and liquid oil would have yielded. And I can conceive that in perhaps as many more years, or less, the whole of the odours would have disappeared. This fact may prove of use to possessors as well as to purchasers; since the former ought not to keep this oil long, as is commonly done, nor the latter to purchase, if they have a choice, that which is concreted.

Of all the oils that I have examined, that of aniseed appears to yield it in even greater quantity than the ottar of rose. A specimen now in my possession, after about twenty-five years, is a solid, and as white as tallow; scarcely containing any liquid oil, while the quantity of odour or odorous matter is also reduced to almost nothing. I cannot discover that there is any sensible difference in this spermaceti from whatever oil it is procured. In the oil of lavender, it appears to require much longer time to be formed in any quantity. In that of bergamot, it is much more abundant in the same time; but it is still in very small proportion compared to that in the oils of anise or the rose.

But, that I may not prolong this paper by noting the oils in which it does occur, I may proceed to say that there are many in which it never is found. Such, among many more, are those of juniper, the fir tribe, or oil of turpentine, and that of cloves. And these oils undergo a very different change from exposure to oxygen. They become first thickened, and finally are changed into resins; while in the oil of the fir-tribe this change is even rapid, as is familiar.

And the general conclusion is interesting to chemistry; since it shews that there are two distinct kinds of essential oil, bearing the same analogy to each other that the fat and the drying liquid oils do in another division of these inflammable substances. Oxygen converts the one set into spermaceti, and the other into resin; just as, in the latter, it renders the one rancid, sometimes also tallowy, while it converts the other into an elastic varnish, a solid. And hence another division of these substances is required. We must make two species or genera of the oils that are evaporated, or perhaps sometimes formed, at or below the boiling point of water; as we have made two of those that do not evaporate under between 500° or 600°, and are obtained by expression, if from vegetables. And it is not unworthy of remark that, in the animal kingdom, we find two oils of this latter nature, which nearly represent the two species or genera of the essential oils. The oil of the spermaceti whale is converted, partially, if not entirely, into spermaceti, by exposure to oxygen, whence also it loses its property of burning readily by age; while that of the Greenland whale is, by the same action, converted into varnish, or becomes a drying oil.

But as it is not my object here to write another essay on this subject, I shall terminate: nor should I have noticed the *discovery* of this Italian Chemist, for the vulgar purpose of claiming it as my own, had he not overlooked the most important conclusions, and indeed had he not left the subject little else than a mere record of a fact, without bearings or interest. If my own papers were so far forgotten as they appear to have been, some time might yet have passed before his discoveries would have been brought to the present conclusions, since they would have formed the point of departure for himself or other experimenters; and there is so much to do in chemistry, that it is ill policy to waste time in discovering what has been discovered before.

On the subject of iodine, every new fact, however insulated, is interesting. Of all the marine plants which I have examined, it is contained in the largest quantity in one which was formerly called Fucus pygmæus, which is now, perhaps, better known to most botanists by its ancient name than by the modern one, which has transferred it to the lichens. And it is best to give it the name that is best known; as the present is not a question of botanical criticism. The very smell of this fucus is almost suffocating; resembling that of iodine when confined in a bottle: but as it is not a bulky plant, though an abundant one where it grows, it never can be a convenient one from which to procure this substance.

I have examined the lichens to some extent, though much less than I desired, in hopes of finding iodine in them, and I wish to point out this investigation to those who have the opportunities which I have now long lost. I have reason to believe that it will be found somewhat widely; but in particular I wish to direct the attention of chemists to the lichen cæsius; a lichen, even under the present nomenclature, unless it has again been changed, under that system which has determined that our memories shall not rust for want of exertion. It is sufficiently common on stones, and ought to be well known to every botanist.

Lastly, having been interrupted in some experiments which I may never be able to renew, I wish that some chemist would examine into the nature of the lilac dye produced from certain lichens, especially from the roccella; since it is not impossible that this colour may be derived in some manner from the substance in question.

To have mentioned dyeing, reminds me of two lilac dyes which I have produced, amongst some other things, which I must defer to a future opportunity, from plants of domestic growth, not hitherto applied to this purpose. The one of these is the berry of the Portugal laurel, and the other the black currant. The simplest process with alum, which I need not describe, is all that is required for either; but, as far as my trials go, the best tint is produced by the former fruit. It is as pure and brilliant a lilac as that from the lichen roccella; but it is far more valuable, inasmuch as it is not reddened by acids, the great evil of all the lichen dyes, nor, as far as I was able to ascertain, by perhaps insufficient trials, so easily injured by the sun. And the preparation of the dye itself is comparatively nothing; so that, as far as this is concerned, the expense would be far less. The question, however, remains—what would be the expense of cultivating this tree for its fruit, compared to that of importing the lichen roccella. This I have no means of ascertaining at present: but it could not be a very difficult question to a nursery gardener; while also, were there a demand, the berries, perhaps, might be furnished cheaper from its native climate. Mr. Macintosh, whose name is authority on such a subject as this, seemed to think, on a general view, that it might be profitable: but I need say no more, where I meant only to offer a suggestion to those who may feel an interest in inquiring further into this subject.

As to the black currant, the produce on any given piece of land is considerable; and there are collateral purposes in this fruit which might render it easier to cultivate it to a profit for the sake of the skins in dyeing. The fruit might first yield marketable jelly, by the usual process, or might even be made into some sort of wine; while the *marc*, in either case, would be the dyer's material.

Let me conclude these miscellaneous notices by pointing out an unobserved formation of a neutral salt, which I find it very difficult to explain. It is the nitrate of ammonia. In the house which I inhabit in London, on returning in December, after an absence commencing with March, during which, I presume, the windows had not been cleaned, the whole surface of the glass was found covered with drops resembling oil. Attracted by this appearance, I examined them, and found that they had a saline taste; and when the room became heated by lighting fires, they crystallized; continuing to alternate between deliquescence and crystallization, according to the state of the atmosphere, until I had collected them for examination.

I do not know of any formation of nitrate of ammonia from the atmosphere, though it probably takes place in nitre beds and dunghills. Yet here, there is a reason for the ammonia; as that for the nitrous acid is familiar. But the alkali, be it what it may, is, in these cases, with formation of nitre, existing in the soil, or at least supposed to be there, while it could not have existed on the glass. Nor was there aught in the room, or in the house, an excellent, large, and especially clean and pure house, without smells or drains, or anything else of this nature, to generate ammonia. If it be so, the ammonia also, as well as the nitrous acid, is formed from the atmosphere; and it is probable that not only this, but potash itself, is thus produced; since in no other way do we know how to account for the formation of nitrate of potash, at least in open soils, where no free potash was ever yet detected. Such is the fact which I had to record. It might be of comparatively small moment by itself; but if it shall ever aid to throw a light on the mysterious formation of nitre, it will not be without its use.

Believe me your very obedient servant,
J. MAC CULLOCH.

We have frequently remarked small radiant and arborescent crystallizations on dirty windows in London, and have found them to consist of *sulphate of ammonia*. This salt, or at least sulphite of ammonia (which becomes sulphate by exposure to air), is an abundant product of the combustion of coal, and to that source its presence in the above instances is probably to be referred. The ammonia alluded to by Dr. Mac Culloch is, doubtless, of similar origin.—*Note by the Editor*.

Observations on the manner of Manufacturing Indigo in the Southern Provinces of India; with some Remarks on its Chemical Changes and Combinations.—By Charles H. Weston, Esq.

Dr. Bancroft introduces his observations on Indigo by the following strong expressions: "The subject of this article is the most interesting, important, and instructive, which can occupy the attention of a dyer or a chemist; the admirable and singular properties of indigo being only surpassed by those of the colouring matter of the murex and the buccinum a little, while it is of much higher practical utility than the latter*." Perhaps there are few men better qualified to form an estimate on this point than Dr. Bancroft, who had removed from the art of dyeing generally so much that obscured it, and had thrown so much light on the changes and chemical characters of indigo in particular.

Not only, however, has it claims, from its peculiar chemical characters, upon the attention of the scientific man, but the important part it now holds in our exports from India must also interest the man of business in its favour. The indigo of the East Indies, which at one time ranked so low in its comparative value, has, by a better mode of manufacture, been progressively improving, and, consequently, as gradually making its way into European markets, till, at length, the exports of the West, of Spanish America, and the West Indies, have been to a great extent transferred to the East.† This is a conquest indeed, but it is a conquest which English *intelligence*, as well as enterprise, has accomplished. To those, therefore, in whose minds the bare mention of the word "theory" is but too apt to call into action every latent prejudice, and to secure to it the most determined opposition, this very striking instance before us, among many others which are daily occurring, of the great advantages resulting from the combined assistance of theory and practice, should lead such, at least, to question the propriety of their hatred to all theoretical knowledge. The tendency of scientific information, as it lays open causes and effects, is, to point out what parts of the process are superfluous, and what may be improved; and, "in proportion as science advances, all the principles become less complicated, and, consequently, more useful; and it is then that their *application* is most advantageously made to the arts*." In the following remarks, I shall endeavour to bear in mind the comparative value of both theory and practice.

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* *Vide* "Experimental Researches concerning the Philosophy of Permanent Colours." By Dr. Bancroft.

† It is, I believe, computed that British India supplies three-fourths of all the indigo brought into European markets.

There will be no occasion for me to make any remarks upon the botanical characteristics of the indigofera tinctoria, as they are already so well known†. Upon the *mode*, however, of manufacturing, or rather of separating, the valuable dye from the indigo leaves, something, it appears to me, need be said; inasmuch, as in works written professedly upon this subject, I have only met with one method of conducting this process, namely, the green or fermenting process; by which is intended the *fermentation* of the plant fresh cut and green in vats properly prepared for the purpose. There is another method the very reverse of this, the production of indigo from the *dried* leaves of the plant *without* fermentation; and this is the method which is to be the subject of the following remarks.

I shall slightly touch upon the cultivation and growth of the plant, making a few observations on its peculiar structure, and its wonderful adaptation, both to the climate and the soil in which it is destined to exist.

Immediately after the great periodical fall of rain, which takes place during the monsoon months of the latter part of October, November, and the beginning of December, the ground is ploughed up and properly prepared for the reception of the seed. The time of sowing will, of course, depend on local situation—the elevated parts sowed before the monsoon has quite passed by, while, from fear of inundation, the low tracts remain untouched till a later period.

The plant requires a light soil, little but timely rain, and much sunshine. Rain should fall immediately after the plant shows itself above the ground, for by it the plant is not only invigorated, but cleansed from those innumerable insects, which otherwise would have fed upon, and destroyed the leaves.

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* *Vide* "Elements of Agricultural Chemistry." By Sir H. Davy.

† See the botanical characters of the indigofera genus in Rees's Cyclopedia, and their classification by Dr. Bancroft.

From this time, comparatively little rain is required, if it fall opportunely, that is at the different stages of the growth of the plant. After the first cutting of the branches, it sickens, and at that period rain is particularly necessary for its proper restoration. The same remarks will, of course, apply to the like periods between the other cuttings.

This plant, unlike the Bengal plant, is only made use of for *one* year, during which the branches are cut off from the stem three or four times, after which the ground is ploughed up for another sowing. But each successive growth of the branches produces an increased deterioration of the qualities of the leaves ; so that one part of the leaves of the first cutting would yield as much indigo as two parts of the third crop.

The produce of the leaves is very precarious, for excessive rain is almost as destructive of the *properties* of the plant, as continued drought is of the plant itself. The rapidity of the growth of plants during much rain, in the temperature of the tropics, is extraordinary, and a proportionate deficiency in all that characterizes the vegetable world necessarily follows. This we find to be the case with all forced vegetables ; and the mildness of the radish of hastened growth, when contrasted with the highly pungent and almost acrid flavour of the slowly and gradually advanced one, may be adduced as explanatory of this observation. Besides this general remark, however, there are other considerations in some measure peculiar to this plant. Indigo contains an extraordinary quantity of carbon, to obtain more or less of which carbonic acid gas must be decomposed; the plant retaining the carbon, and transpiring pure oxygen gas *. But as this important change can only be effected when the plant is under the direct and immediate influence of light, rainy weather, by intercepting the sun's rays, prevents the accomplishment of this very important part of vegetable economy. Hence it is, as it is practically well known to manufacturers, that the indigo plant, however fine and luxuriant, as is the natural result of much rain, is very deficient in *produce*, and a similar loss is experienced even if the plant, without the fall of too much rain, has grown up under *cloudy* weather. Sunshine, much and continued sunshine, is essentially necessary for the proper exercise of those secretory organs by which this peculiar drug is formed and perfected.

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* There are, indeed, some plants which must derive the *whole* of their carbon from the atmosphere. Sir H. Davy remarks, " Many plants that grow upon rocks or soils containing no carbonic matter, can only be supposed to acquire their charcoal from the carbonic acid gas in the atmosphere." In the sandy soil of India there certainly cannot be enough of vegetable matter to supply a proper proportion of carbon.

This plant is destined, therefore, to exist under a burning sun and much deficiency of rain; how then is it to obtain its necessary supply of water? Nature, who invariably so well adapts the means to the end, has, in this instance, displayed her usual wisdom. The root, instead of being short, and branching off in different directions, and reaching to various depths, is one continuation of the stalk, which, decreasing gradually to a point, after the manner of the common garden radish, descends perpendicularly into the ground, to the depth of about three feet. While, therefore, the plant is necessarily exposed to the continued heat of a broiling sun, it is deriving benefit from the moisture of that part of the soil, which always remains uninfluenced by any external heat. I before stated that the nature of the soil should be light and sandy; and the non-conducting powers of the sand, in reference to caloric, afford a sufficient reason for the complete protection of the root. I have often found that, while the surface at mid-day was too hot to allow the hand to remain in contact with it, the temperature of the sand, at even a short distance beneath the surface, was gratefully cool*.

There remains another reason why the plant can endure drought so continued. As dew is the condensation of the aqueous vapours exhaled by the sun during the day, the quantity of dew, *cæteris paribus*, will always be in the direct ratio of such heat; and hence it is that, in clear still nights, after such unclouded weather, the fall of dew within the tropics is immense. Now, this water, so precipitated during the absence of the sun, completely envelopes the entire plant, and is copiously taken up by those absorbing vessels, which form a part of the wonderful physical structure of the leaves of plants. Thus is it constantly restored to the full exercise of all its powers, ready to receive the renewed action of the sun's rays, so necessary for effectuating the decomposing process before mentioned*.

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* A good instance of the non-conducting power of sand is given in the following extract:—"The red-hot balls employed by the garrison of Gibraltar to destroy the Spanish floating batteries, were carried from the furnaces to the bastions in *wooden* barrows, with only a layer of sand interposed, and this was found sufficient to prevent the balls, though in a high state of incandescence, from setting fire to the wood."—TILLOCH'S *Philosophical Magazine.*

But plants, like animals, transpire as well as absorb moisture, and equally vary in their power of doing so. Less quantity of water is, therefore, necessary for the growth of one genus of plants, whose transpiration is limited, than for the wants of those of another genus, whose transpiration is more extensive †. Now, although the extent of transpiration may be dependant upon, and be in proportion to the absorption of water, and *vice versa;* and, therefore, as the absorption is extensive the transpiration must be so likewise‡; yet may there not be some *balance* in favour of the absorbing powers over the transpiratory powers of the plant §? These appear to be some of the methods which Nature adopts to meet those apparent difficulties, the continuance of an unclouded sky, and the constant and necessary supply of water.

It may be further remarked, that in the growth and formation of this plant, beside the water essential to the solution, or carrying in suspension different vegetable, alkaline, or earthy substances, to the plant—much water is not wanted: because, as hydrogen in the ultimate products of indigo*, forms rather an unimportant part, the decomposition of water must be proportionably limited.

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* "In very intense heats, when the soil is dry, the life of the plants seems to be preserved by the absorbent power of their leaves; and it is a beautiful circumstance in the economy of nature that the aqueous vapours are most abundant in the atmosphere, when it is most needed for the purposes of life."—Davy's *Agricul. Chemistry.*

† *Vide* Ure's Chemical Dictionary, article "Vegetable Kingdom," and Brande's Manual of Chemistry, vol. i.

‡ See this stated in Dr. Thomson's extensive observations upon "Vegetables," in his System of Chemistry.

§ We learn from Mr. Bonnet's "Researches concerning the Use of Leaves," the great extent to which vegetation can be carried by the water absorbed *solely* by the leaves, although *transpiration* was, at the same time, going forward. (*Vide* Thomson.)

We now return to the cultivation of the plant, and view it as gradually advancing to maturity. As it approaches this period, the colour of the leaves changes from a light to a dark green—yet this change is not gradual, but is quickly acquired a short time before the branches are fit to be cut. Much judgment on this point is required, for the evils arising from premature cutting are great, and much of the good effects of antecedently seasonable weather would be neutralized by such a mistake. The consequences are, a deficiency of produce, and an unequal absorption of oxygen in the beating vat. When the plant is fully matured, the branches are severed from the parent stem early in the morning, and spread out in the sun till the afternoon. By this time they are so desiccated, that the leaves are easily separated from the branches, by simply beating them with a stick. The leaves, so separated, are housed in warehouses, closely packed, and well trodden down by natives.

The importance of *properly drying* the leaves will be easily understood, when we learn that the leaves are not immediately used, but are kept so packed for one month. Moisture very soon produces fermentation, involving either a partial or total destruction of the colouring matter. Deficiency of produce is, of course, the natural result of such change, and where no such bad effects have been felt, the use of *ill-dried* or green leaves produces an effect in the beating vat, similar to that which is consequential on premature cutting.

The leaves, however, when properly dried, do not remain

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* These, according to Dr. Ure are—

Carbon	71.37
Oxygen	14.25
Hydrogen	4.38
Azote	10.
	100.

for one month without suffering a very *material change*, and accomplishing a very important object. We find, at the expiration of this period, that the colour of the leaves has passed into a *light lead colour;* by additional keeping, the lead colour gradually darkens until it becomes black. Experience has found that the leaves will not give out any colouring matter to cold water, until the first change has commenced to take place,—that the maximum quantity of indigo is to be obtained when the lead colour is effected, and that, from this period, a loss in the quantity of indigo always accompanies, and is in proportion to the extent of such further changes. But, perhaps, the *necessity* of keeping the leaves, and the *nature* of the important change so accomplished during that period, will be better proved and elucidated by a perusal of some memoranda of a few experiments I made in India.

No. 1. " Digested some *green* indigo leaves from the plant, fit for immediate cutting, in *cold* water, for twenty-five minutes ;—the slightest *yellowish* extract, only, was produced.

2. " *Green* mango, and other leaves, digested in cold water for two hours, gave no extract.

3. " *Green* mango, and other leaves, *boiled* for an hour and a quarter, gave a clear reddish brandy colour to the water, and the leaves of a creeping plant similarly steeped, produced a yellow tinged with green, not clear, but leaving a sediment.

4. " Boiled the *green* indigo leaves taken from a plant fit for *immediate* cutting, for two hours and ten minutes. The liquor was decidedly a *greenish* yellow. This left on the filter the *smallest* quantity of indigo, and the solution, after twenty-four hours, became darker, and a little more indigo was precipitated. Both precipitates, however, most inconsiderable.

5. " Green solution of indigo in cold water, prepared from the *dried* leaves taken from the heap which had been one month in the warehouse, was kept in rapid ebullition for one hour,

and water was added to supply the loss by evaporation;—the *whole* of the indigo was precipitated, and the supernatant liquor was of a clear dark morone colour*.

6. "Indigo leaves, taken from the ripe plant (part of which was used in experiment No. 1), after *ten days' keeping*, gave to the *cold* water, in which they were digested for twenty-five minutes, a fine *grass green colour.*

7. "Mango, and other leaves, well *dried*, and *kept* spread out on paper for ten days, gave to *cold* water a deep brandy colour.

8. "*Boiled* for a short time, some *green* leaves taken from the ripe indigo plant, and obtained, by filtration, a brandy colour solution with a greenish tinge: half part of which (No. 1) was exposed to the air, and the other half (No. 2) was poured back over the leaves equally exposed to the air: No. 1 remained unchanged, and, when boiled, became only a darker brown, and gave no precipitation of indigo: No. 2 subsequently *fermented*, and *afterwards* became a good green.

9. "Green leaves, digested in *cold* water for one quarter of an hour, filtered, and kept exposed to the action of the atmosphere for fourteen hours, remained *unchanged.*

Cold water, allowed to *remain on the leaves* for fourteen hours, became green."

Now, the legitimate inferences to be deduced from these experiments, I conceive to be these:—

1. That as the green leaves of plants give no colour, and the leaves of the indigo plant no blue colouring matter, to water by simple infusion (Exp. 1 and 2), and the same leaves, after desiccation, and exposure to the atmosphere for many days, will, under the same circumstances, give out what is essentially the colouring matter of the leaves (Exp. 5, 6,

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* The effects of boiling upon this supernatant liquor will be mentioned hereafter.

and 7), the warehousing the indigo leaves for procuring indigo by the dry process is *necessary*.

2. That the blue principle does *not* exist in the leaves *ready formed*, or in a state of separation (Exp. 8 and 9), but requires some new arrangement of its component parts to effect such separation;—and
3. That the *absorption of oxygen* is necessary either to produce such new arrangements, or to complete some incipient arrangement of its constituent parts (Exp. 4, 5; No. 2 of 8 and 9), and, so to separate the blue extract, as to make it soluble in water, and therefore the *change* effected by one month's keeping seems occasioned by an *absorption of oxygen*.

With such a view of the important changes which thus occur, we see that, chemically speaking, there is no such difference in the two processes as we might be led to imagine from the varied manner of *manufacturing* the indigo. The silent and gradual change which passes upon the dried leaves in the storehouse, is precisely analogous to the absorption of oxygen which precedes, and, in this instance, continues to be simultaneous with fermentation*. It might be further remarked, that the injury, arising from keeping the leaves after the acquirement of the "lead colour," to which I have before alluded, has its parallel in the injury sustained by excessive fermentation in the Bengal process. The period, during which the leaves are kept, appears, indeed, to be the only time in which the leaves could have received oxygen to *such an ex-*

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* Although vinous fermentation can be carried on completely excluded from the atmospheric air (*Vide* Brande's 'Manual of Chemistry,' vol iii.), this does not seem to be the fact in the present case of common fermentation, or partial decomposition. Dr. Bancroft, in his first vol. p. 185, writes,—"But it was fully ascertained by Dr. Roxburgh, that a copious *absorption* of air from the atmosphere did occur; and that oxygen did combine with the basis of indigo in a considerable degree *during* fermentation, was manifested by the progressive changes which, as usual, constantly took place in the colour of the liquor during the fermentation until it acquired a full green, and even a bluish colour, the froth, or scum, becoming, more or less, blue at the same time." See, also, p. 176.

tent; because, during the growth, as long as the sun's rays are exerting their direct influence upon them, they copiously give out oxygen gas, and, during the absence of the sun, the exhalation of carbonic acid gas necessarily employs another portion of oxygen[*].

Before quitting this part of the process, I will here state another experiment on the solution of *green* leaves in cold water, because, while it at first appeared completely to upset the conclusions I had deduced from former experiments, it did, upon subsequent consideration and examination, strongly confirm the truth of them.

Green leaves steeped in *cold* water for fifteen minutes began to tinge the water with a *greenish* yellow. Leaves from the same plant, after five hours' boiling (the loss of water by evaporation being constantly supplied), gave out a proper extract of indigo; and from another portion of leaves, after twenty-four hours' digestion in *cold* water, the *whole* of the indigo was precipitated, and the supernatant liquor clear, and of a good "brandy colour." These were unexpected results, and they seemed to throw a doubt over the consequences of every one of the former experiments: my first inquiries and examination were, therefore, directed to the plants from which the leaves had been taken. I found the plant had been cut *one month* previously, and that the leaves were some few under leaves remaining on each plant of the *former* crop. Now, as the leaves had become ripe one month before, they had arrived at, and passed the maximum capabilities of their physical powers; and their organs had long ceased to perform their proper functions in decomposing carbonic acid, and of exhaling oxygen gas. We know that fruit, when ripening, absorbs oxygen[†], although allowed to remain on the tree, and the same tendency doubtless exists in the whole vegetable world in a greater or less degree.

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[*] I have qualified this remark by adding, "to such an extent," because it is not intended to intimate that either during the day or night the balance of oxygen is not *in favour* of the plant. The contrary, in both cases, is clearly proved by the experiments of Saussure, as mentioned in Thomson's 'System of Chemistry.'

[†] M. Berard of Montpellier has showed that fruits, in ripening, convert the oxygen of the air into carbonic acid.—Davy's *Agricul. Chem.* See also Dr. Smith's *Botany*, p. 155.

The leaves subjected to the above experiment had assumed a much darker green than leaves possess at the proper time for cutting, and as they had passed beyond the full period of strength and health, they could have no other tendency but to absorb oxygen[*]. They had then, consequently, been undergoing, for at least three weeks, the same process, though perhaps in a less degree, which the leaves in the warehouse had experienced, and had absorbed on the plant, that given quantity of oxygen which is necessary to render the colouring matter of the leaves soluble in water. Such a view of the subject reconciles all apparent difficulties.

The difference in the effects of ebullition upon the green leaves, in Experiment No. 4, and upon these in the last experiment, in reference to extent, can be easily explained, on the supposition of a previous absorption of oxygen; since, within certain limits, matter has its affinity for oxygen increased in a kind of geometrical ratio by the previous absorption of oxygen.

From this last experiment we get possession of this important fact, that in practice, the age of the plant, and the time for keeping it, should always be in an inverse ratio; that is, *if from fear of rain the plant be cut too soon, the leaves should be kept a little longer; and if from want of sufficient sunshine the cutting be deferred till after the plant is fully ripe, the leaves will not require to be kept so long.*

We now suppose the indigo leaves, after their due keeping, ready to be transferred to the "steeping vat[†]," where they are commixed with water, in the proportion of about one volume of leaves to six of water, and allowed to remain for two hours. The great affinity of indigo for oxygen is here very manifest, in the quick change of the colour of the leaves which float on the surface, and are exposed to the action of the atmosphere, to a blackish blue, when contrasted with those below which remain unchanged. On this account the vat is, every now and then, "turned," that is, the floating leaves are immersed. This steeping part of the manufacture constitutes the characteristic difference between the green and the dry process. In this instance, it is a simple *infusion* of the leaves without fermentation; in Bengal, the branches, together with the leaves, are steeped and allowed to *ferment*.

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[*] *Vide* Sir H. Davy's *Agricul. Chem.* p. 207—209.

[†] A vat is an uncovered reservoir, built of brick, and lined with stucco prepared from burned shells: it is usually about thirty feet square, and twenty-six inches deep.

After two hours' infusion, the water which, from the solution of imperfectly-oxygenized indigo, has acquired a fine green colour, is allowed to run off from the leaves through strainers into the "beating vat." Two hours are generally supposed to be sufficient to extract all the colouring from the leaves, and the following experiment seems to confirm this supposition*.

"Refuse leaves of indigo taken from the vat, and quickly rinsed, to remove any indigo externally adhering to them, were digested in cold water for one additional hour. They gave to the water a pinkish colour, but no *green* tinge, from which was obtained, by a solution of acetate of lead, a delicate whitish yellow precipitate." My observations, of course, refer to leaves properly kept and properly dried; because, if prematurely put into the vat, the time necessary for the entire extraction of the indigo would be much greater. But the green liquor cannot be allowed to remain so long upon the leaves, because a partial precipitation of indigo would take place before it passed into the beating vat.

In the beating vat the solution is agitated by the paddles of ten or twelve natives for about two hours, during which time the fine green liquor gradually darkens to a blackish blue. No precise time can be laid down for the continuance of this part of the process, as the absorption of oxgen which is thus effected, and hastened by beating, and the consequent continual exposure of some new portions of the liquor to the atmosphere, depends on circumstances, such as the former preparation of the leaf, and the immediate influence of the sun*.

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* Whatever may be the necessity of making use of *hot* water for obtaining indigo from the leaves of the nerium tinctorium, or the advantages to be derived from its use in the fermentation of the indigofera tinctoria, which Dr. Roxburgh mentions, no possible advantage can be derivable from such management in the dry process; because, as we find that the solvent power of cold water is quite sufficient to extract the whole of the blue colouring matter, an increase of that power, by the addition of caloric, could only have the effect of dissolving other substances which are foreign to the dye.

The only criterions to guide us are,—the peculiar black hue of the liquid, the whiteness of the froth thrown up by beating†, and the incipient separation of the particles of the indigo. This last effect can be easily seen by placing a small quantity in the palm of the hand, and more distinctly by pouring some into a white plate.

At this time lime water is thrown into the vat, and well immixed with the liquor. The whole is then allowed to rest for three hours, and the supernatant liquor, which is, or ought to be, of a fine bright Madeira colour, is allowed to run away from the precipitate, by cocks placed at different heights. The indigo is then removed from the vat to the covered part of the manufactory called the laboratory, where it is put on a strained cloth, and allowed to drain throughout the night.

But as the beating process is the most material part of it, so it is here that we most sensibly feel any improper management with regard to the leaves. Premature cutting of the plant, imperfect desiccation of the leaves, and even the previous occurrence of much cloudy weather, while the plant was growing—shew their bad effects in the necessity of a much more lengthened beating, and in imparting to the froth a green colour which, otherwise, as the oxygen was absorbed, would, by a precipitation of the indigo, have become white. When the two first evils are to a greater extent, no beating will precipitate all the indigo, and, consequently, much will be lost. The supernatant liquor, instead of being of a fine Madeira colour, will remain a dark, dingy Madeira colour‡. The following experiments will give force to this observation:—

"A quantity of Madeira colour liquor left after the manufacturing of indigo improperly dried, was reduced by boiling to one-third its bulk. The colour was deepened, and the smell of it highly saccharine, and *blackish indigo was precipitated.*

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* It has been before remarked that the vats are uncovered, and therefore exposed to the direct rays of the sun.

† This must be regarded with some limitation, as we shall see soon what will prevent sometimes the whitening of the froth.

‡ Dr. Bancroft confirms this remark.

At this time the yellow extract of the liquor appeared to begin to separate. The liquor, after the removal of the precipitated indigo, was boiled for other seven hours, supplying the loss occasioned by evaporation: *more indigo* was precipitated, combined with some of the extract."

Again, "Another portion of the Madeira colour liquor was first exposed to the action of the atmosphere for seventeen hours, and then beaten for two hours. The solution became greenish by the additional absorption of oxygen, and a *little indigo* was precipitated."

As, therefore, the quantity of the precipitation depends upon, and is in proportion to, the oxygenation of the indigo, the importance of carrying on this beating process to its maximum to obtain the whole colouring matter is evident; but there is another extreme to be avoided.

When indigo is combined with a certain quantity of oxygen, its colourless base acquires a blue colour; but after this point is passed, and a further combination of oxygen has taken place, the colouring matter passes from a blue to a blackish-blue, and the indigo is technically said to be "burnt." If beating be carried on still further, the granulated indigo becomes specifically lighter, and a partial re-solution of it follows, and it is therefore unavoidably lost. Each of the different effects here mentioned will be more clearly pointed out and understood, by perusing the following memorandum:—

"A green solution of indigo from the vat was agitated for ten hours. The supernatant liquor, instead of being a fine clear Madeira colour, became like burnt umber—resulting from a partial re-solution of the indigo. The indigo became specifically lighter, and remained floating in the menstruum. The whole was filtered, and the indigo properly pressed and dried, and the cake when broken was *black*."

Beating, strictly speaking, is not essentially necessary to the formation of indigo, but is rendered expedient by the *accumulation* of the green solution in one place, and the consequent non-exposure of a great part of it from the influence of the oxygen of the air*. I make this remark, because I have found that portions of the green liquor could be *completely* oxygenized by bare exposure to the atmosphere, without agitation. "Made a solution with indigo leaves, filtered it, and divided it into two equal parts, one part of which (No. 1) was exposed to the direct influence of the sun's rays, and the other (No. 2) was kept in a dark place. The first portion was considerably in advance of the other throughout the process, so that when No. 1, in two hours and half was completely precipitated, leaving the supernatant liquor a fine clear Madeira colour, No. 2, in the same time, was but partially precipitated, and the supernatant liquor remained greenish. After longer exposure, however, the whole indigo of No. 2 was also precipitated."

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* This remark will serve to render evident the expediency of precipitates, although, as we shall soon see, they be not necessary in the small

By this experiment we learn, not only that a complete precipitation can take place without beating, but without the assistance of lime-water. We also learn that sunshine can hasten the beating process, as I before remarked; the great affinity of indigo for oxygen enabling it to separate and combine with that gas, which always co-exists with the least refrangible rays of light*.

Having said thus much on this most important part of the manufacturing process, I shall now make some observations upon the yellow extract of the leaf, which has been denominated "the supernatant Madeira-colour liquor." We see that the indigo leaf imparts to water two distinct colouring matters—the blue and the yellow; and that a solution of the latter, when a complete precipitation of the indigo has been effected, is bright and clear. This extract appears to have many qualities in common with the blue. From experiments already detailed, it has, like indigo, an affinity for oxygen (although, indeed, in an inferior degree,) its colouring matter is capable of being deepened by the absorption of oxygen, and by a further absorption in part precipitated. Its affinity for indigo is also great, for it

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way Dr. Roxburgh well describes their utility, when he says:—"If well chosen, and in proper proportions, they forward the operation *much*, causing a *larger* produce than could be had without them." His advice as to the time for using them, is worthy some attention.

* *Vide* "Light and Caloric."—THOMSON'S *Syst. of Chem.*, and URE'S *Chem. Dict.*

is always combined in some degree with the granulated indigo: and when the yellow dye ceases to be given out from it to *cold* water, *hot* water will extract another portion, as we shall by and by see; and again, after the whole process is completed, pounded indigo will afford more colour to boiling water.

I have found that a solution of acetate of lead will throw down from this Madeira-colour liquor a fine yellow precipitate; and that this, when filtered and gently dried, remained unchanged after an exposure to the air for nine months. But a more delicate yellow colour can be procured, by adding acetate of lead to a filtered green solution of indigo from the steeping-vat, when the indigo is first precipitated combined with the salt, leaving the supernatant liquor clear. To this solution, filtered, again add acetate of lead, and the precipitate will be a fine yellow, brighter than that separated by beating.

I endeavoured to fix that colour on cotton-cloth, and found that, by frequent alternate steeping the cloth in this Madeira-colour liquor, and drying it, and so saturating it with the colouring matter, and subsequently steeping it in a solution of acetate of lead, an insoluble precipitate was effected between the fibres of the cloth; and the yellow colour was imparted. But I have not followed up this experiment, and therefore can say nothing of the permanency of this colour alone, or when protected by an after dip in the blue vat for obtaining a green; or how the colour may be affected by aluminous and other mordants. I have nevertheless thought that some notice should be taken of this experiment, imperfect as it really is, that it might serve as a hint to some scientific indigo-planter, whose numerous opportunities will enable him to try whether the immense quantities of colouring matter now daily allowed to run to waste can be turned to any advantage.

I have said that the indigo leaves produce two dyes—the blue and the yellow; strictly speaking, however, there is yet another colour which may be thus obtained. The refuse leaves taken from the vat and well washed, to remove all colouring matter adhering to them, when boiled for an hour and a half, will render the water yellow, tinged with green. This water, kept in full ebullition for two hours (supplying the loss occasioned by evaporation), will, when filtered, afford a precipitate, which, when dried, will in colour be a dun-slate, and in quantity perhaps about equal to the blue extract such leaves have produced. This observation, as it can lead to no practical advantage, is made for the man of science, rather than the man of business. The precipitate appears to be very impure, and mixed with some of the finer parts of the fibres of the leaves.

We now revert to the manufacturing process. The precipitated indigo left in the vat was transferred to the cloth-strainer within the laboratory, and allowed to drain throughout the night. On the following morning, the indigo is put into the copper, with a quantity of hot water, and raised to ebullition. As the mass is gradually heating, a quantity of scum rises, which is immediately removed, and as soon as the whole is brought to the boiling point, the fire is withdrawn. The contents of the copper are retaken to the strainers, and the drained indigo is then divided into small portions, and each portion well worked by the hands of the natives. This manipulation is continued till the indigo be freed from air-bubbles, and has assumed a close, fine, and smooth appearance. The whole is then carried to the pressing-boxes, which are usually square, and of sufficient depth to leave the cake about two inches and a quarter in thickness. By means of a powerful screw, the water is separated from the indigo, which is then left in a large square cake. This is subsequently cut into small cakes, which, gradually dried in the shade, and carefully protected from the hot land-winds, are thus rendered fit for exportation.

This last part of the process I have described as succinctly as possible, because I felt I was travelling over a path already well trodden ; but I could not wholly pass it over, as I wished to make some observations upon the *uses* and *effects* of boiling as they have occurred to my mind, and which I have not seen mentioned in any work upon indigo. Dr. Bancroft, indeed, speaks of boiling indigo in copper vessels, but this was boiling indigo in a weak solution of soda, for the *sole* purpose of removing impurities, and, therefore, differs from that we are now considering.

The first effect of boiling appears to be *a condensation of* *the particles of indigo*, because I have found that the aggregate quantities of indigo and water, when taken out of the copper, shew a much greater deficiency than could possibly, I think, have resulted from loss by evaporation.

Another advantage arising from boiling is, I should say, the *removal of much extraneous colouring matter* from the indigo. I have before observed that the affinity between the yellow dye and indigo is such, as always to combine with the precipitated indigo. Cold water, whether affused over, or digested upon, this precipitate, extracts no colour, but the hot water in the copper is always as strongly tinctured with the brandy colour as the supernatant liquor of the heating vat.

Another advantage derivable from partial boiling is *an improvement of the hue* of the indigo, occasioned, I infer, by an additional absorption of oxygen. Experience has shown the observant manufacturer that indigo, when boiled, possesses a purplish hue, which indigo carried from the strainers to the pressing boxes does not possess; experiments before detailed prove that oxygen is absorbed during ebullition, and even to such an extent as to precipitate indigo—and oxygen we know to be the direct cause of colour;—we may therefore fairly infer that the improvement of tint in the copper is occasioned by its absorption. The following memorandum will perhaps throw some light upon this part of the process, and give us some caution as to the extent to which it should be carried:—

"Boiled indigo for seven hours after its own proper boiling and constantly supplied the loss of water: thrown upon the filter, the separated liquor was a good brandy colour. The indigo pressed and dried proved to be a *dead* blue."

From this we learn first, that after the fine purplish hue has been acquired, we have arrived at the maximum of improvement in colour, and that any further accession of oxygen deteriorates the colour. And secondly, that the oxygen so absorbed by boiling, is in a proportion far less than during the beating process *.

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* Compare this experiment with that in which the effects of excessive agitation were related.

Another use of boiling is the throwing to the surface a quantity of scum, which is usually said to consist of resinous impurities ; but of this I would affirm nothing, inasmuch as I have never examined the chemical qualities and nature of this substance.

I have now given a pretty full account of the dry process of manufacturing indigo ; in detailing which, my object has been to give some chemical rationale of the different parts of the process, to venture suppositions respecting the nature of those changes to which the plant is necessarily subjected ; and at the same time to point out the grand characteristic difference between the steeping and the fermenting modes of manufacturing.

This paper, I am aware, cannot be considered sufficiently comprehensive in matter, as it by no means imbodies the whole process, practical and theoretical ; and I doubt not but, to the chemist, many of the preceding pages afford abundant specimens of much that is superficial, both in observation and in scientific knowledge. I was, however, alone induced to offer any remarks upon such a subject, from the conviction that the almost entire absence of information would give to them a kind of artificial interest which otherwise they could not have possessed. It is but justice, indeed, to myself, to add also, that these memoranda were originally never intended for the perusal of a second person, but that they formed the first experiments I ever made on the interesting properties of indigo. These I had fondly hoped, at some future period, to have extended so far as to give myself a full insight into the whole process, for much additional examination is absolutely necessary. Some of the advantages, for example, of boiling the indigo, as they have struck my mind, have indeed been alluded to, but more experiments and closer attention may bring to light other and, perhaps, more important effects than some of those already mentioned. So, also, with respect to the other parts of the process, which I consider rather as an outline to be filled up by future observations. With proper apparatus, and a little perseverance (as almost every part of the manufacture may be conducted under receivers, &c.) not only may the nature of the changes which are constantly taking place be clearly and unquestionably explained, but the quantities of gases absorbed and evolved be correctly ascertained.

Circumstances have prevented the possibility of my renewing such delightful employments; I therefore leave my observations, unsatisfactory as they may be, as hints to others, who may have both opportunities and abilities to investigate the subject throughout all its bearings.

An Account of the Chain-bridge erected at The Hyde, Ingatestone, Essex, 1829.

Having proposed to myself the erection of a bridge over a piece of water in my private grounds, it occurred to me that one of suspension might be constructed upon common chain, if it were sufficiently strong, and would, at the same time, be much cheaper than either in wood or brick.

After consulting with some persons whose practical knowledge of chains was much greater than my own, and having calculated, as well as I was able, the probable weight of the proposed joists, flooring, and side pieces, I procured a proper quantity fit for my purpose, ready made, from one of the principal dealers in London. The breadth of the water is fifty feet, and, consequently, the suspended flooring is the same, and the width of the walk is four feet. By as accurate a computation as I could make, I found the weight would be from 3-4ths of a ton to a whole ton, making the flooring of inch deal-boards, and the other wood work and parts of the dimensions stated below. The suspending wires do not weigh more than two hundred weight.

The chain I have used, I was told, would support, in a perpendicular direction, singly, one ton and a half. The bridge complete is represented in p. 316. The dimensions of the different parts (shewn in the same and following page) are as follows:

The main chains, A, *Fig.* 1, three pounds to the foot, are seventy-five long each from the places where they join the anchorage.

The joists, B, *Fig.* 3, are of heart of oak, four feet six inches long, and in substance three inches by two, bearing the weight edgewise. In the

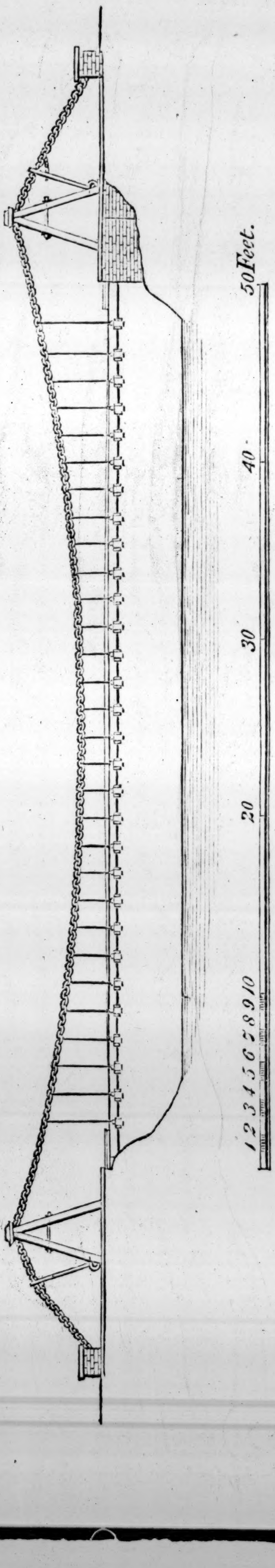


Fig. 1.

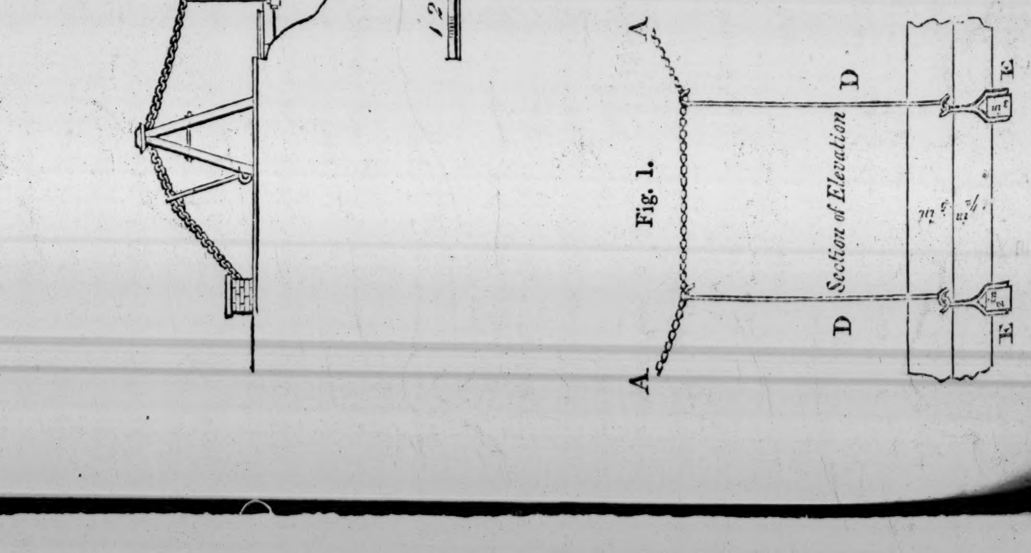


Fig. 2.

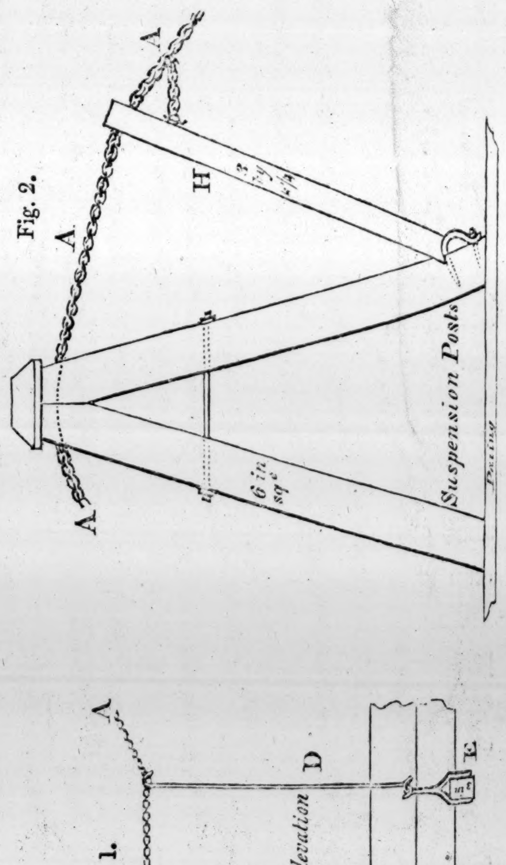
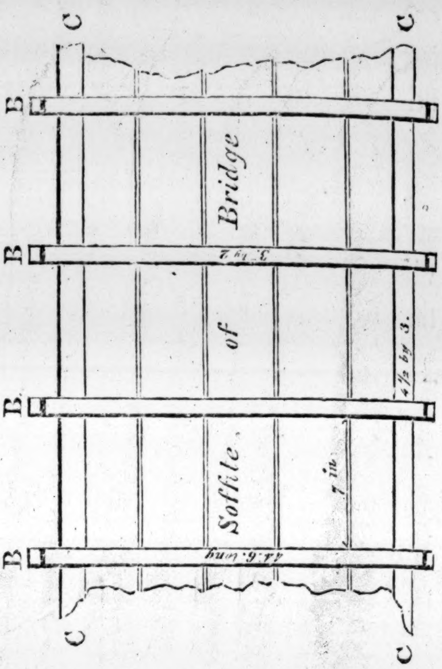


Fig. 3.



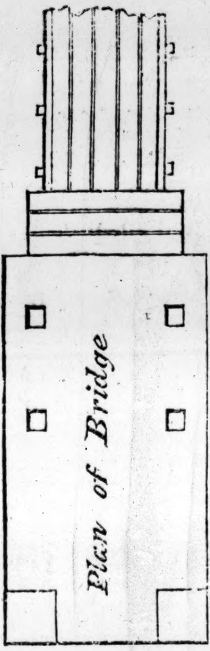


Fig. 6,

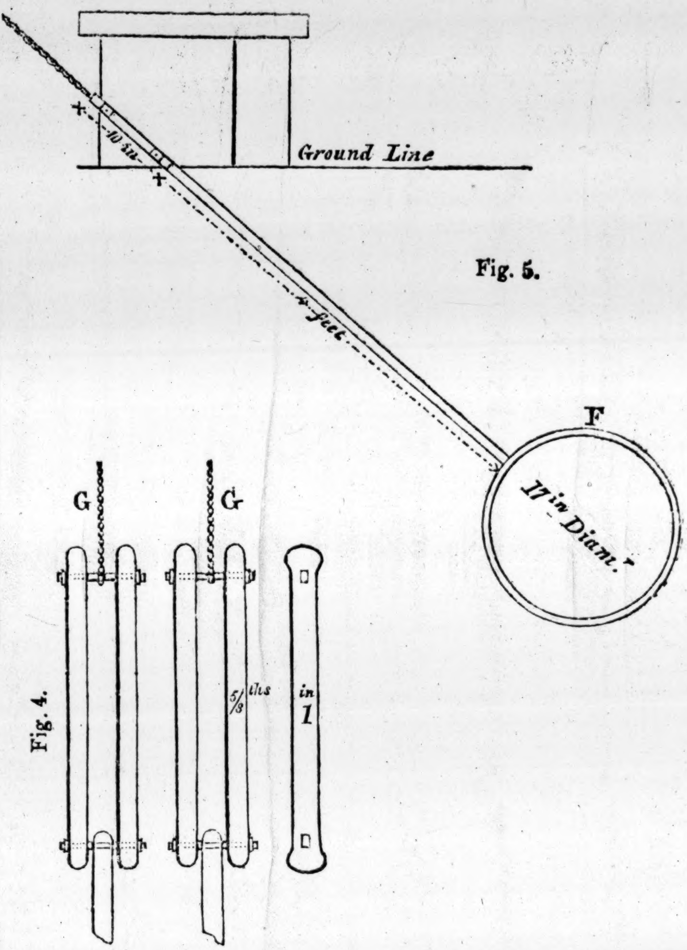


Fig. 5.

Fig. 4.

under side of these, two inches from each end, is a notch for the purpose of receiving a stirrup, (in order to keep it in its place,) in which they rest. These joists are fifteen inches apart.

The side pieces, C, *Fig.* 3.—The joists being placed in their stirrups, there are laid over them, and just within them, four pieces of deal, three inches by four and a half in substance, and twelve feet long each, having notches cut in the lower edges, at equal distances of fifteen inches, to receive the joist, and are made fast to them by a screw passing downward into the joist; these I have called side pieces, and their use is to keep the joists in their places, and steady.

The suspension wires, D, *Fig.* 1.—The chains being properly placed, and secured, iron wires 3-8th inch in diameter, of proper length, having a hook turned at each end, after the manner of S hooks, were put on; one end of each wire is hung upon a link of the chain, and on the other is hung the stirrup.

The stirrup, E, *Fig.* 1, is made of the same sized wire, with a loop, or hook, at the top, and receives the end of the joist on its flat side, falling into the notch described before.

The anchors, F, *Fig.* 5, if they may be so called, are in fact old garden-roller stones, round each of which is a strong bar of iron, in substance one inch square, welded again to itself, so as to form a ring, in which the stone lies horizontally. This bar is brought out to the length of four or five feet, and turned into a very strong hook at the end; the stone with the hoops (for there are two at each stone, being one for each chain) are buried in the ground three to four feet deep, and rammed in, presenting above ground a portion of the iron bar and the hook.

The chains, G, *Fig.* 4, are fastened to the anchorage by means of plates of iron somewhat similar to the links of the chains in the large suspension bridges. Two parallel plates, of similar shapes and equal lengths, with a hole at the end of each, are placed one on one side and one on the other of the anchor-hook, and a bolt as large as the hook will carry, passed through the three, and screwed close together by a nut. A similar bolt is run through the other ends of the parallel plates, and the last link of the chain being placed between them, are screwed together in the same manner. The plates are ten inches long, and one inch by 5-8ths in substance.

There are two chains on each side of the bridge, and the joists are hung alternately, each on two chains only. Thus, the first joist hangs on the outside chain, on the left hand, and the inside chain on the right hand; the next on the inside chain on the left hand, and on the outside chain on the right hand; the third hangs as the first, the fourth as the second, and so on, so that each chain supports half the weight of one side of the bridge, and, consequently, 1-4th of the whole. The piers (*Fig.* 2), over which the chains pass, are of wood, heart of oak, five inches by six in substance, and set into the ground two feet and a half. They are put with their tops leaning together, at such an angle as to make the base of the triangle half its perpendicular height; they are braced together under ground, and bolted together at three feet and a half from the surface of the land, and also, again, immediately below where they meet. A moveable stone cap protects them, and the grove in which the chains lie, from wet.

They have been now in use nearly two years, and have not varied from their original position the 1-8th part of an inch, as appears from a mark with a chisel, made when first put up.

When the bridge was first erected, the chains were brought in a *direct line* from the top of the piers to the anchor-hooks. But, then, the angles formed by the descending chains on each side of the piers, with the perpendicular axis of the triangle, were not equal, and the pressure was necessarily thrown more upon the inner post than the other. To have corrected this by lengthening the chains, would have made them inconveniently long. I therefore took the bridge off the chains, and placed an oaken stay, H, (*Fig.* 2,) resting its lower end on a strong iron bracket, I, (*Fig.* 2,) at the foot of the outer post, and put it in a direction parallel to the inner post, making its length just sufficient to raise the chain to an angle, externally, equal to the inner one. At the same time, finding that the vibration of the bridge was very considerable, owing to its length, the suspended flooring (which is exactly fifty feet), and its great lightness, I put an additional stay on each side, in one piece, forty-eight feet long, over the other four side pieces, and bolted them together, which has had the effect of stiffening it, without adding much to its weight, which I conjecture does not now much exceed a ton.

The beauty of the bridge is great, and though not absolutely necessary, being over a piece of water in a private pleasure ground, is very useful and efficient. The whole cost of materials and workmanship did but little exceed thirty pounds, This account I have put together, which, it is feared, is much wanting in technical or scientific accuracy, to shew how easily and cheaply foot bridges of this description may be erected; and of materials always to be had. If the posts which support the chains, the most perishable of all the parts, should in ten or fifteen years decay, so as to endanger the fabric, ten pounds in money and three days in time would set it up again.

Upon a rough estimate, a wooden bridge of the same span would have cost from eighty to one hundred pounds, and a high arch probably from one hundred and fifty to two hundred pounds.

I am, &c.,

JOHN DISNEY.

The Hyde, May, 1829.

On the Construction of Fire-proof Buildings. *By* Ignatius Bonomi, *Architect.*

One great, and perhaps the principal cause of the destructiveness of fires in large buildings, is the want of arched surfaces of incombustible materials. This has been disastrously exemplified in the destruction of the choir of York Minster, where the roofs of the aisles, which are solidly arched with stone, suffered no injury; while the choir-roof, although much more raised above the action of the fire, has been entirely destroyed by it: and there is little doubt but that the whole roof of the church would have suffered the same fate, if its continuity had not been interrupted by the walls of the tower.

The use of arched surfaces of solid materials cannot be too strongly recommended. In no part of our church-architecture is the skill of the contrivers so conspicuous, as in the art employed in the construction of the vaulting, in order to procure strength and reduce the lateral pressure, which they effected by a frame-work of stone, of sufficient depth, converging towards the points of support, and by filling in the intervals with thinner material. They thus imitated the structure of the vegetable leaf, in which the fibres centre upon the stem, as the ribs or frame-work of the arch on its support; at which point, also, the buttress meets the ribs, to counteract the lateral thrust.

In many instances, to render the construction lighter, surfaces of *brick* are used between the stone ribs; and, where extreme lightness is required, *hollow pots* (cylindrical within) of well-burnt clay, would prove an excellent substitute for bricks. The use of these was not unknown to the Romans, who also employed *pumice;* this porous material possessing the additional advantage, when combined with good cement, of rendering the arched surface one united petrification, opposing (in consequence of its firm union) little lateral pressure, comparatively, against the sustaining walls.

A very frequent occasion of fire is the frequent necessity for the repair of *lead gutters,* which requires the use of braziers on the roof. This necessity would, in a great degree, be obviated by the adoption of gutters of *cast iron,* or of *solid* *lead* cast in troughs, and having spouts at proper intervals to carry off the water. The action of the sun upon gutters, which by expanding the metal, is the principal cause of their failure, might also be much diminished by a thin covering of stone or slate, sufficiently perforated for the percolation of water. This contrivance would also have the advantage of preventing the lodgment of snow, which is apt to freeze in the gutters and occasion the water to overflow, to the damage of the building, and particularly of the timbering of the roof.

Another cause of fire is, the use of roofs of timber, especially when connected with the roofs of other buildings. In such cases the substitution of *iron* for wood would afford security; and if, in the use of iron, care is taken to make the rise or pitch of the roof sufficient to prevent indirect strains, and to *tie in* the feet of the main supports or triangles, there can be no danger of failure, provided the strength of the iron is proportioned to the weight it has to carry. If, however, the building has an arched under-roof of solid materials, and care is taken to prevent the necessity of continual repairs to the lead-work, the danger of fire, even from a roof of timber, is very considerably obviated. In adverting to the use of a substitute for wooden roofs, it may be proper to specify that in some instances the slated surface might be carried upon *cross walls*, supported by the arches and divisional works of the building.

In a paper written for the express purpose of recommending precautions against fire in the construction of public buildings, it will hardly be deemed foreign to the purpose to state the impropriety of collecting into one room great numbers of books, or works of art. Even if security is obtained against fire from without, there is always danger to be apprehended from the use of lights within; and one spark igniting might occasion the destruction of the whole contents. The present collocation of the libraries of the advocates and writers to the signet in Edinburgh presents a striking instance of this kind of risk.

The means which may be recommended for securing such valuable collections from destruction by fire, are, in the first place, to use *incombustible materials* in the construction of the buildings containing them; and, secondly, to subdivide these collections into suites of separate rooms, which may be connected by wide and high *doors of metal*, rendered ornamental by plates of bas-relief, made to open upon pivots, and poised with mechanical skill, so as to be easily moved. By these precautions, should a fire unfortunately destroy the contents of any one room, it might be prevented from extending to the adjoining apartments.

It is not here intended to enter further into the details of construction; or to refer to the use of arches upon cast-iron beams and sheets of metal, &c.: but it is desirable to explain, that an excellent surface for interior finishing may be obtained, by using (instead of lath and plaster) a lining of brick *detached from* the exterior walls. This not only affords a security against fire, but has the additional advantage of interposing a medium of air between the inner and outer walls, which, by its slow conducting power, will prevent the interior of the building from partaking of the variations of the outer atmosphere, and consequently, in cold weather, will avoid the precipitation of moisture on the inner walls. To conclude, the examples afforded us in the ancient buildings of our own country, and those of Rome, present to the architect's contemplation a source of study on the subject of solid vaulting, in which, however dissimilar the forms, he will discover the application of the same mathematical principles of construction, combining strength, lightness, and economy of material.

Durham, March, 1829.

On the Early History of the Steam-engine.

A PAPER on this subject, and particularly on the rival pretensions of France and Great Britain, published by M. Arago, in the "Annuaire du Bureau des Longitudes" for 1829, has excited more attention than might have been thought due to so hacknied a topic. The cause of that attention will, perhaps, be found in the preface by which the paper is introduced, and of which the following is a translation.

"The steam-engine has rendered such important services to industry and navigation, that we cannot be astonished at the eagerness with which various nations have disputed the honour of the invention. Yet it will not be heard without surprise, that in England alone one hundred thousand copies of the works in which this historical question is discussed, have been sold within a very few years. Such striking success is due, I doubt not, principally to the lively interest which the steam-engine must naturally excite in a country where it is met with at every step; but may we not suppose that self-love has had some share in producing this effect? Ask the member of the House of Lords, and the most simple artisan; the merchant whose speculations have led him into every region of the world, and the farmer who has never passed the borders of his own county; traverse the manufactories of Birmingham, Manchester, and Glasgow, and the humblest workroom of a cottage; every where you will hear that the first inventor of the steam engine was the Marquis of Worcester. Every where this name will be cited, and it will be followed by those of Savery, of Newcomen, of Beighton, of Watt, of Hornblower, of Woolf, &c. &c. Literary men, and those who cultivate the sciences have, in general, opinions no less decided. If you open the recent Encyclopædia of Dr. Rees, you will find this passage: 'The steam-engine follows next to the ship in the order of inventions, but in an English Encyclopædia it will take the lead, from the circumstance of its being wholly invented and brought into general use by our own countrymen.' And then, as if this were not sufficiently clear: 'The steam-engine has been invented by a small number of individuals, all of them Englishmen.'

"The celebrated Professor John Robison, of Edinburgh, is quite as positive. He says, 'the steam-engine was, beyond all doubt, first invented by the Marquis of Worcester, in the reign of Charles the Second.' Then, after having refuted by arguments, which I will examine, the pretensions of those French authors who affect to mix up the name of Papin with the history of the steam-engine, Robison declares, 'That he does not hesitate in giving the honour of the first and complete invention to the Marquis of Worcester.' A philosopher, not less illustrious for the profoundness of his knowledge, than for the vastness of his erudition, Dr. Thomas Young, has joined his imposing testimony to those I have quoted. According to him the Marquis of Worcester is the first inventor of the steam-engine, the first who has used the pressure of vapour as a prime mover. In the rapid view which he takes of the ameliorations which this machine has successively undergone, no names appear but those of Englishmen. I could also cite the able Professor of Mechanics at the Royal Institution, Mr. Millington; a distinguished member of the new University of London, Dr. Lardner; the author of an esteemed Treatise on Mechanics, Mr. Nicholson, &c. &c.

" Decisions so numerous, so positive, the just reputation of the works from which I have quoted them, would seem to exclude the shadow of a doubt. When, therefore, at the desire of the pupils of the Polytechnic School, I attempted, some years since, to trace the chronological series of improvements in the steam-engine from its origin till our own time, I expected, I say it without equivocation, to have only English mechanics to cite. This was, however, a mistake. Our neighbours on the other side of the channel are not the sole, neither are they the first inventors of the steam-engine."

The last paragraph of this preface will explain the reasons for the extraordinary notice which has been taken of the paper. It has been supposed that the dispute was as new to every body, as M. Arago describes it to have been a few years since, to him. But the fact is, that the dispute, if it may be so called, is as old as the steam-engine itself, and it has been kept constantly alive for the last hundred years, as every one acquainted with the works of Belidor, Bossut, Montgery, and many others on the continent and in England, must be fully aware. The first of these writers alludes to it in his " Architecture Hydraulique," published in 1725, and concludes thus : " A proof that the steam-engine had its origin in England is obtained from the fact, that all the engines erected out of England have been the work of Englishmen." After this testimony by a Frenchman, writing when the facts were fresh in the recollection of the scientific world, it is almost too much to charge all the English writers (one only excepted) with having sacrificed the truth to their national prejudices ; and especially when this is coupled with such an extraordinary demand on their credulity, as is made by the assertion that *a few years since* M. Arago imagined that none but English names had been associated with the history of the steam-engine. If M. Arago were thus ignorant of the facts, the circumstance is evidence of suppression or neglect, not so much on the part of the English, as on the part of the French writers, who on this subject are numerous and voluminous, and with whom M. Arago could not have been unacquainted. If, a few years since, M. Arago did not know that France had produced the steam-engine, we may presume that it was a fact not generally known in France. If then France had forgotten her own work, England might have been pardoned for not recollecting it.

The best reply to M. Arago's paper will consist in a brief exposition of every known step in the application of elastic vapour to the production of motive forces; from which may be deduced the real value of those facts which the English are incorrectly charged with suppressing, and from which also will be seen how unnecessary was the following declaration of M. Arago: "Mes citations, mes analyses, seront exactes; on peut y compter." It is not usual for scientific writers to premise that they are not going to falsify their extracts; the name of M. Arago would have been considered a sufficient security against such a proceeding. It will, however, be found in the sequel, that some of the *citations* are not quite *exact*, and that the variations are always in favour of the writer's hypothesis; but it would never have been supposed that the variations were made designedly, even without the singular protestation before quoted.

The earliest known suggestion for the use of steam is one made about one hundred and thirty years before Christ, by Hero of Alexandria, and published in his "Spiritalia." Its principle and mode of action will be understood from *fig.* 1, where *a* represents a globe moving freely on its axis in such a manner as to permit the constant introduction of steam from a detached boiler. The steam escapes through the bent tubes *b*, *b*, and gives, by its reaction, a rotary motion in the direction of the arrow.

Fig. 1.

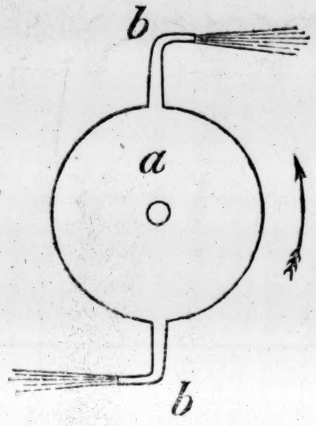


Fig. 2.

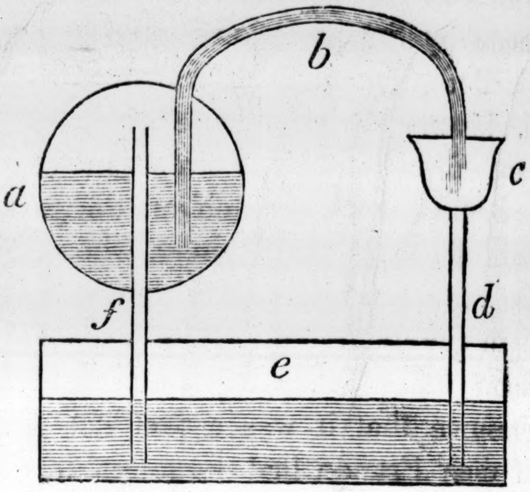
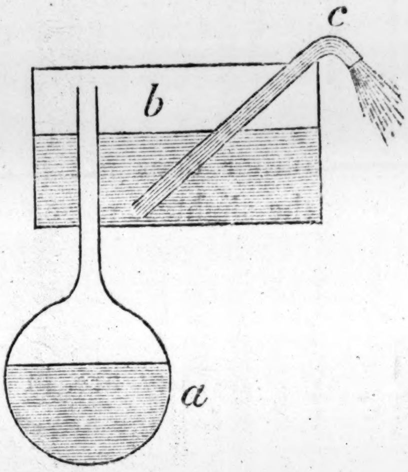


Fig. 2, describes another apparatus of Hero, in which *a* is a globe half filled with water, which is partly converted into vapour by exposure to the sun. A pressure is thus occasioned on the surface of the water, which is consequently driven up through the syphon *b*, into the cup *c*, whence it descends by the pipe *d*, into the close vessel *e*, also half filled with water. When the globe *a* cools, the water it contains is relieved from the greater part of its pressure by condensation, and the water rises from the vessel *e*, through the pipe *f*, to supply what had been driven over by the elasticity of the vapour. Thus the water is alternately expelled from, and restored to, the globe, by the alternate production and condensation of elastic vapour.

It is obvious that the engraving in this case is a mere diagram; that the apparatus would require a valve opening downwards in the pipe *d*, and another opening upward in the pipe *f*. They are omitted in Hero's diagram, most probably for the reason, that the use and application of such valves were then too well known, and too obvious to require notice.

The first of the two contrivances above given is quoted by M. Arago. The second he omits, though both are contained in all the editions of Hero's well known treatise. In this case, therefore, the *citation*, if not *inexact*, is *incomplete*, and it happens that the second apparatus anticipates the principle of both the contrivances on which M. Arago builds his theory, that the steam-engine is a French invention.

A translation of the work of Hero was published in Italian, by Baptista Porta, in 1606. He repeats Hero's inventions, and adds the following of his own, illustrated by a diagram, in which is shewn the furnace for heating the water. In *fig.* 3, the furnace is omitted; in other respects it is a copy from the work of Porta. The boiler, *a*, has a long neck, which passes through the bottom of the close cistern *b*. A bent pipe or syphon, *c*, is closely fitted into the top of the cistern and descends nearly to the bottom. When the fire is lighted under *a*, the steam ascends into the cistern, presses upon the water, and forces it up the syphon, *c*, whence it runs in a stream.



This is altogether a very extraordinary suggestion, and considering how well known must have been the works of the celebrated Baptista Porta, it is surprising that the project was not sooner reduced to practice than is found to be the case. The contrivance is a great improvement on that of Hero, (not because artificial heat is employed, for it is obvious that Hero proposed to use solar heat, merely for the sake of variety, but) because the steam from heated water in one vessel is made to drive up cold water from another vessel, instead of driving up the heated water from which the steam is produced. With this important modification the apparatus resembles that proposed by the Marquis of Worcester in 1663, excepting only in the extent of its power. The difference is merely one of degree. Porta's drawing and description, put into the hand of any intelligent workman, could not fail to realize all that the Marquis of Worcester hinted at in regard to the steam-engine.

This apparatus of Baptista Porta is neither described nor alluded to in the paper of M. Arago, which professes to give every suggestion of importance; of course it was unknown to him; but he cannot avail himself of this apology without recollecting that English writers on the steam-engine might have had the same apology for omitting French contrivances, which he must resort to for neglecting that of an Italian. It is not admitted that the English writers have omitted to notice any French invention of the slightest consequence; but if they had, the case of M. Arago and the Italian will, for the future, suggest to him other causes for such an omission than national prejudice and self-love.

Only nine years after the publication of the work of Baptista Porta, that is to say, in 1615, appeared a treatise on *Moving Forces*, and especially on the means of raising water. The author does not profess to give new inventions, nor is there the slightest reason for ascribing to him *that* which will presently be considered. He says, "there *are* five means of elevating water above its level," which means he then describes, and which were then all of them old, most of them 2000 years old. They are, in fact, brought forward as *existing* inventions, and some are expressly referred to their authors.

The *first* mode of raising water, he says, is by the syphon. This instrument was older than Hero.

The *second* is by the well-known operation of capillary attraction.

The *fourth* is by the action of compressed air, illustrated by Hero's fountain, and ascribed to Hero.

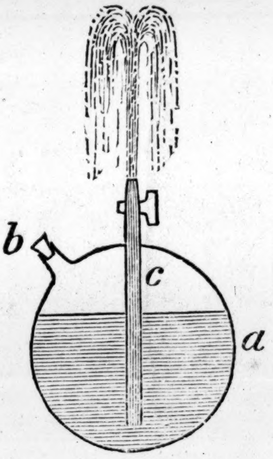
The *fifth* is by animal labour applied to machinery, such as the screw of Archimedes, or the pump of Ctesibius.

It would evidently be absurd to call the writer of the book in question the inventor of any of the four apparatus above described. He himself makes no greater claim to the third than he does to the others, and the works of Hero and Porta shew that he *has* no claim; yet it has been very improperly conceded to him by several English writers;—by several of those writers whom M. Arago charges with suppressing the evidences of the French title. The reader will guess that the work above quoted is a French work, and that we now approach the period at which France invented the steam-engine. The work was written by Salomon de Caus, whose name and pretensions were familiar as household words to the English, through the medium of English books, for years before M. Arago rescued them from the oblivion in which he says English prejudice had buried them. But to return to the fact, that the steam-engine is on the point of being invented in France by Salomon de Caus.

The invention of this mighty agent, and its announcement to the world, fully justify the epithet which M. Arago gives to the inventor, when he calls him the "modest Salomon de Caus."

The announcement forms the *third* of the means which De Caus says *are* in existence for raising water.

"The *third* means is by the aid of fire, for which various machines may be made;" and he proceeds, "I shall here give the demonstration of one. Let there be a globe, *a*, having a valve, *b*, to introduce water, and a tube, *c*, soldered into the upper part of the ball, and descending nearly to the bottom; after having filled the ball with water, and well closed the valve, place it on the fire; then the heat, acting on the ball, will cause the water to ascend through the tube, *c*."



Nothing can be better than this as an illustration, in a general treatise, of the means by which water may be elevated; and nothing could be more ridiculous than to put this forth in 1615 as an invention; for, during the previous fifty years, Europe had been deluged with translations of Hero, and only nine years before had appeared a full description of the apparatus of Porta, to which this is so obviously inferior, that it would be an insult to the reader to say why.

It must, however, be recollected that Salomon de Caus is not himself chargeable with the absurdity of calling this contrivance either new or useful; he most probably omitted the cold water vessel of Porta, because he equally well explained what he wished without it; namely the simple and well-known fact, that the elastic power of steam might be used to raise water.

It is scarcely credible that the hot water fountain described in *Fig.* 4, neither possessing nor pretending to the slightest feature of novelty or utility, is what M. Arago calls the invention of the steam-engine in France, and the publication of which by De Caus entitles him to be called "un homme que la postérité regardera, sans doute, comme le premier inventeur de la machine à feu."

This case, as far as regards the claim made in behalf of France and De Caus, may now be left to the reader; but it remains to consider it in reference to M. Arago's protestation, " mes citations seront exactes, on peut y compter."

The diagram, *Fig.* 4, is accurately copied from the work of De Caus; the diagram given by M. Arago as an extract from the same work is represented in *Fig.* 5, where it will be seen that the tubes *b* and *c* are indefinitely elongated, and the jet of water omitted. This is, of course, an accidental variation, but it renders the assertion of M. Arago, that the apparatus is a "proper machine for draining," a little less amusing than it would have been without this example of the *exactness* of the *citations.*

Fig. 5.

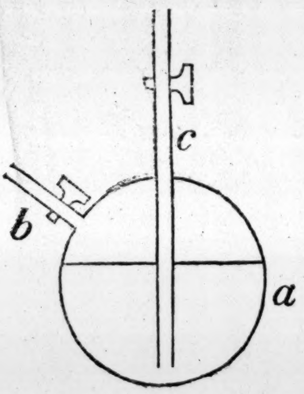


Fig. 6.

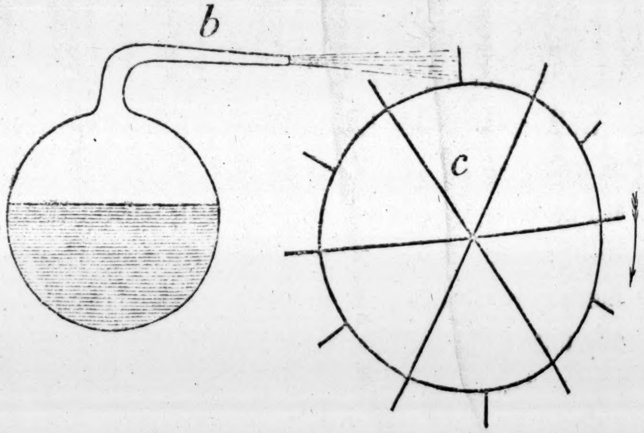


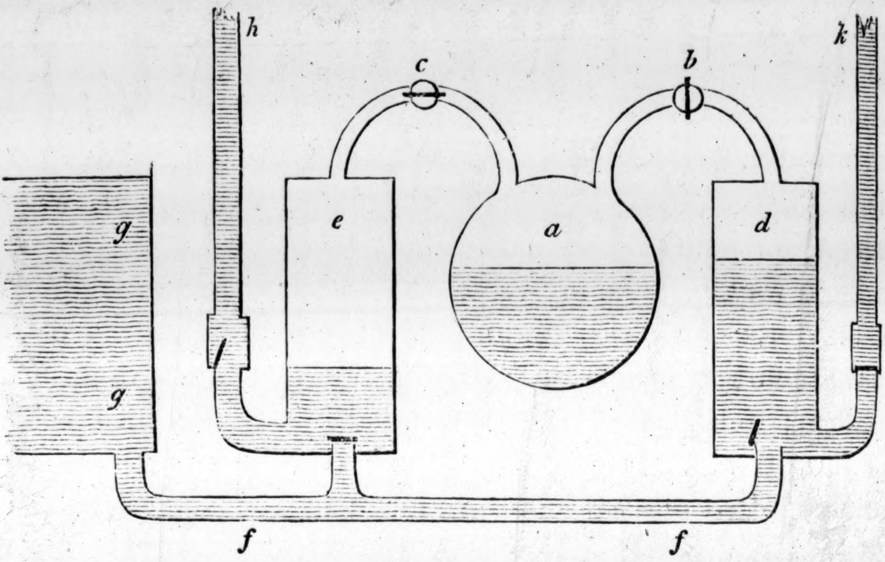
Fig. 6 describes an apparatus proposed in 1629, by Branca, an Italian. The boiler *a* terminates with a bent tube *b*, from which the steam issues, and impinges upon the vanes of a wheel *c*, which receives motion in the direction of the arrow. There is very little novelty in this contrivance; the principle does not differ from the first apparatus of Hero, and the practical effect would probably be inferior.

In 1663 appeared the "Century of Inventions," by the Marquis of Worcester; the following is a copy of that numbered 68.

" An admirable and most forcible way to drive up water by fire, not by drawing it or sucking it upwards, for that must be, as the philosopher calleth it, *intra sphæram activitatis*, which is but at such a distance. But this way hath no bounder, if the vessels be strong enough: for I have taken a piece of a whole cannon, whereof the end was burst, and filled it three-quarters full of water, stopping and screwing up the broken end, and also the touch-hole, and making a constant fire under it, within twenty-four hours it burst, and made a great crack; so that having a way to make my vessels so that they are strengthened by the force within them, and the one to fill after the other, I have seen the water run like a constant fountain-stream forty feet high; one vessel of water rarefied by fire driveth up forty of cold water. And a man that tends the work is but to turn two cocks, that one vessel of water being consumed, another begins to force and refill with cold water, and so successively the fire being tended and kept constant, which the self-same person may likewise abundantly perform in the interim between the necessity of turning the said cocks."

This apparatus is, in principle, the same as Porta's. The merit of the Marquis of Worcester consists in having suggested the use of highly elastic steam, in exceedingly strong vessels, so as to raise water to a useful height, as forty feet. He has also the merit of urging the adoption of this plan on the large scale, whereas the schemes of Hero and Porta were mere experimental toys. It is most probable that the Marquis never tried this apparatus on a large scale, notwithstanding his assertion to the contrary; but that his description may easily be realized there is no doubt, though M. Arago denies it, and although he says that Mr. Stuart could not realize it, otherwise than by grouping together two of the vessels of Salomon De Caus, and working them alternately. Mr. Stuart's description is very incorrect if it be as M. Arrago states; for the globe of De Caus ejects hot water, whereas the Marquis of Worcester proposes that one vessel of *heated* water shall raise forty of *cold* water. This M. Arago insinuates it is impossible to do by adhering strictly to the description of Lord Worcester. The annexed diagram appears, however, to realize all the conditions. Let *a* (*Fig.* 7,) represent the boiler communicating by the pipes *b* and *c*, with the cold water vessels *d*

Fig, 7.



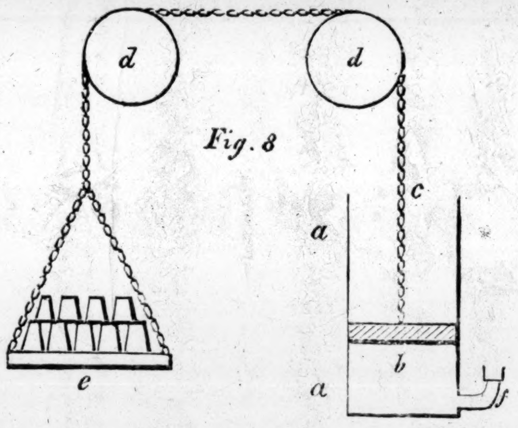
and *e*, which latter communicate by the pipe *f f*, with the water to be raised, *g g*. The pipes *h* and *k* ascend to the reservoir, forty feet above, and their lower ends enter the cold water vessels *e* and *d*. The two cocks, *b* and *c*, are those which are described to be opened and shut alternately. The four valves in the pipes *f*, *h*, and *k*, are precisely such as had been used for nearly two thousand years in pumps. The vessels *d* and *e* are in fact forcing-pumps, in which steam is substituted for a solid piston. The cock *c* is in the diagram open, the steam is therefore forcing the water out of *e*, and up the pipe *h*. The cock *b* is closed, and the vessel *d* is cooling; the water tends therefore to enter it both from the cistern *g g*, and from the pipe *k*; the arrangement of the valves, exactly like those of a pump, prevents the latter and permits the former: therefore, the vessel *d* as it cools is filled from the cistern *g g*. This action would no doubt go on alternately, by maintaining the fire, and alternately opening and shutting the cocks *b c*; and it is quite certain that this proposal more nearly resembles the first steam-engine known to have been made, than any prior invention. In this diagram, the most unfavourable case is taken for the Marquis, because there is nothing in his description to forbid our concluding that he would have availed himself of what was then well known, the vacuum formed by the condensation of the steam. He would then have placed the vessels *d* and *e* twenty or thirty feet above the water *g*, and in this shape it would be very nearly a perfect anticipation of the first working steam-engine.

On the whole, very little credit is due to the Marquis of Worcester. The majority of his hundred projects are either absurd or puerile: some are impossible to realize, many are not worth realizing, many contain no novelty, and several have a taint of the perpetual motion. All this is so evident, that it would hardly have been thought worth while to mistranslate the Marquis, in order to make his one rational project as absurd as many of the others, yet this M. Arago does.

The Marquis says, " my vessels are strengthened by the force within them." This has been usually translated by French writers, " mes vaisseaux sont fortifiés intérieurement." M. Arago translates it, " des vases qui *se fortifient* par le *développement* de la force intérieure." He says that he is aware that the rendering of former translators is more reasonable than his own, but that it ought to be made extravagant to correspond with the other projects of the Marquis. M. Arago has certainly succeeded in making the Marquis absurd, for he supposes him to mean that the stronger the steam, that is to say, the greater the strain to which they are exposed, the stronger will be the vessels; and, in order to bear out this assumption, he alters the whole passage. Lord Worcester says, " the vessels are strengthened." M. Arago translates it, " des vases qui *se fortifient*." Lord Worcester says, " by the force within them." M. Arago translates this, par le *développement* de la force intérieure;" the word " *développement*" is gratuitously introduced to assist the first misrepresentation, and to help the insinuation that it was proposed to strengthen the vessels by the strength of the steam; for the word *développement* is unintelligible as applied to a constant source of strength; it must mean something which fluctuates in the vessel, and there is nothing which does this but the steam.

To proceed with the historical series, it may be mentioned, that about this time Otto Guericke was engaged in those experiments and speculations which led to the invention of the air-pump,—to a better knowledge than had hitherto been obtained of the air's weight and elasticity, and particularly of the powerful effects of that weight and elasticity when excited by an exhausted, or partially exhausted, space.

Otto Guericke, in his work on the subject, published in 1672, describes numerous contrivances for exhibiting or using mechanically the weight of the air. One apparatus he describes similar to that in *Fig.* 8, where *a a* is a cylinder, to which is fitted a piston, *b*, attached to a chain, *c*, which passes over the pulleys, *d d*, and supports a loaded scale, *e*. A pipe, *f*, enters the cylinder near the bottom, and to this pipe a pump is to be attached, by whose means the air may be withdrawn from the lower part of the cylinder, which will, of course, cause the piston to be forced downwards by the unbalanced pressure of the atmosphere. In this way Guericke says that a boy of twelve or fifteen years old, acting on the pump at *f*, may raise a weight, however large, in the scale, *e*, provided only that the diameters of the pump and the cylinder, *a*, be properly proportioned.



This, it will be perceived, is an anticipation of the principle of Bramah's hydrostatic press; not that it at all interferes with the credit due to Mr. Bramah for that beautiful invention, because he claimed no discovery of new principles, but a better application of those which were old.

In this apparatus of Otto Guericke, which is fully described by a very large picturesque engraving in his work of 1672, we have a complete explanation of the principle of obtaining power from a vacuum formed under a movable piston. It is quite clear, also, that the mode of producing a vacuum, by means of condensed steam, was known and published at this time. If, therefore, any one had proposed to Guericke, to make the vacuum by condensing steam, instead of by a pump, there would be no vast stretch of ingenuity in the suggestion. But suppose Guericke to reply, "How shall I apply the steam?" Imagine, then, that he is thus answered :—"Place a little water in the bottom of the cylinder, then set the cylinder on a fire till the water is converted into steam, and the piston is at the top; secure the piston from descending, and carry the whole apparatus to wherever the work is to be performed, then fix the cylinder, attach the piston to the point where your power is to be applied, and as the cylinder cools, your work will be done." If Otto Guericke were a man of business, he would reply, this will cost about ten times as much as exhausting the cylinder by hand. So that this suggestion, besides possessing the smallest possible quantity of originality, would have the property of being utterly inapplicable to useful or economical purposes.

About the year 1685, Monsieur Papin communicated to the Royal Society and others, various devices for using compressed and rarefied air to elevate water in jets or pipes. In none of these is there a particle of novelty, as will be evident to whoever will compare them with the works of Hero and his translators. He contrived an apparatus of this sort, which he decorated and placed as an ornament on his chimney shelf, but he concealed within the shelf a pipe communicating with an adjoining room, through which air was forced into the ornament, and produced a jet of water. This he invited the members of the Royal Society to see, and challenged them to discover how the jet was produced. The mysterious ornament was described and published without reference to the concealed pipe, and some foolish persons imagined that the perpetual motion had been discovered; till at length Monsieur Papin disclosed, what it is probable all the scientific world had long known,—that the effect was produced by some concealed artifice.

Shortly after this, Papin proposed to convey motion from one place to another, at great distance, by means of air-tight tubes communicating between cylinders containing pistons.

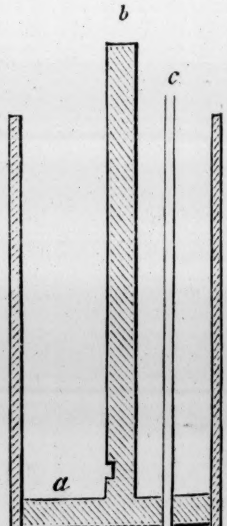
He thought that, in this way, a motion in one cylinder could be transferred to another at the distance of a mile or more. This is something like the scheme which has been lately before the public, of starting a carriage from London by means of an air-pump at Brighton.

Any person who could seriously propose to work a piston in a cylinder at one end of Oxford-street by means of an air-pipe, communicating with a second cylinder at the other end, must be ignorant of what ought at that time to be fully known, namely, that air was inert like other matter; that it was elastic, and that in pipes its motion was retarded by friction.

In 1669 Papin copied Otto Guericke's contrivance, and describes it as his own invention; whereas the only change he makes, is in causing the descent of the piston to express oil from seeds instead of raising a weight. This is a mere plagiarism.

From about 1685 to 1700, therefore, it appears that Monsieur Papin was engaged in forming contrivances to employ the force of exhausted or compressed air upon pistons in cylinders.

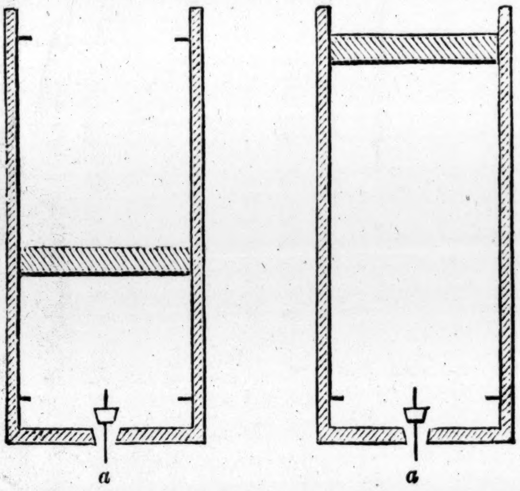
In 1688 he suggested the use of gunpowder to form the vacuum by its explosion, and in 1690 he proposed to form the vacuum by steam from a small quantity of water placed in the cylinder.



In *Fig.* 9, the cylinder of Papin is described; *a* is the piston, and *b* is a rod attached to it. There is a notch in the rod just above the piston, into which a catch, fixed on the top of the cylinder, enters and retains the piston at the upper end till the catch is released. The tube, *c*, permits the escape of the air when the instrument is first used. The fire is brought under the cylinder, and retained there till the water is converted into steam, and the piston driven up. The piston is then secured by the catch, the fire is removed, the cylinder gradually cools, and at length the piston, being set at liberty, descends under the pressure of the air.

This is the apparatus on which M. Arago founds the completion of the claim of France to the honour of having invented the steam-engine. It was never made as a working machine, and it does nothing more than illustrate a well-known physical fact. That a vacuum was left by condensed steam was known from the time of Hero at least. That the air, or some other power, would force a liquid or solid body into a vacuum was equally well known.

But let us see how M. Arago treats this matter. The section introducing this subject is headed "Denis Papin," and immediately under are placed the two diagrams, *Fig.* 10, which the reader of course concludes are the portraits of Papin's invention. But it will hardly be believed they are nothing of the sort; they are portraits of the apparatus of the Englishman, Newcomen, made fifteen years afterwards. M. Arago gives six pages of description of this apparatus, in which he brings the steam into the cylinder from a boiler through the valve *a*, and which he says is very clearly described in the Leipsic Acts for 1688, and in a work of Papin's in 1695; and he details certain experiments made with a cylinder which weighed five ounces.



At the bottom of the sixth page M. Arago proceeds: "The water which furnished the steam in these *first* experiments was not contained in a separate boiler—it had been placed in the body of the cylinder, upon the metal disc which closed the lower end. This disc Papin warmed to convert the water into vapour; and it was the same disc which cooled when the fire was removed in order to produce the condensation." From this we conclude that, as the apparatus described by M. Arago was not that used by Papin in his *first* experiment, of course it must be the apparatus used in Papin's second or third or last experiments. Nothing, however, would be more erroneous than such a conclusion. Papin never experimented on such an apparatus as M. Arago ascribes to him, he never even suggested such a one.

It is for the reader to judge of the fairness of ascribing to Papin in 1690 the invention of Newcomen made in 1705, without the slightest hint that such a substitution has been made. M. Arago thus sums up his notice of Papin:—

"Papin was the first to imagine a steam-engine with a piston.

"Papin was the first who saw that aqueous vapour furnished the means of obtaining a vacuum in a *large space* (grande capacité).

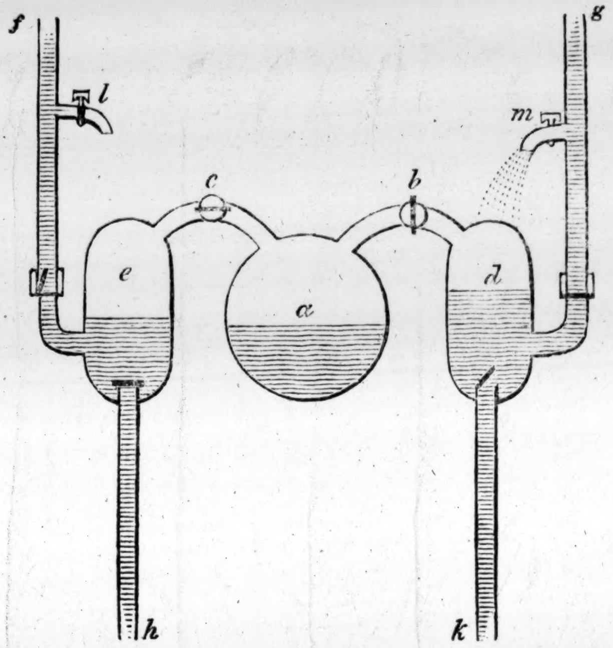
"Papin was the first who thought of combining in the same fire machine, the action of elastic vapour with the property of the same vapour to become condensed by cold."

If Papin's apparatus be a steam-engine, he is entitled to such a share in the invention of that agent, as appears due to the novelty of his contrivances, and to their practicability. The reader may easily judge of each.

Papin was not the first by two thousand years who saw that condensed aqueous vapour left a vacuum. What differences there are between large and small spaces in this respect remain to be shewn. The capacity of a five ounce cylinder hardly deserves to be called a large space.

Papin's apparatus does not use both the elastic force and condensation of the steam; and if it had, he was not the first who combined in the same machine the elastic force of the vapour and its condensibility; for Hero's apparatus does the same thing.

We proceed to the year 1699, when M. Savery exhibited to the Royal Society a model of the first engine, which is known to have been realized on the large scale. This model and its mode of action will be rendered intelligible by the diagram *Fig.* 11, where *a* is a boiler communicating by the bent pipes



b and *c* with the cold vessels *d* and *e*: *f* and *g* are the pipes through which the water is forced out of the vessels by the steam, and *h k* are he pipes by which it enters these vessels from the well or cistern beneath. The arrangement of valves is precisely like that supposed in the Marquis of Worcester's, *Fig.* 8, and the only difference is, that here the cold water ascends into the vessels *d e*, instead of flowing from the same level, and that the cooling of the vessels *d e* is hastened by suffering cold water to run upon them alternately from the cocks *l m*. This last contrivance, the artificial condensation, is the only point of complete novelty about Savery's machine. He has, however, the rare merit of combining several principles which had been merely suggested, in an apparatus which presented to the world for the first time an effectual working machine for raising water by fire.

But we must now hear M. Arago.

His section headed "Savery," thus begins: "We have no proof that Salomon de Caus ever constructed his steam-engine;" I might say as much of the Marquis of Worcester. "That machine of Papin, in which the action of the vapour and its condensation are successively employed, was executed only on a small scale. The reader will form an exact idea of Savery's, if he recollects that of Salomon de Caus, and attends to the following considerations."

Thus the only description which M. Arago gives of the first working steam-engine mankind ever saw, is by stating verbally without any diagram, where it resembles and where it differs from what he calls the steam-engine of Salomon de Caus. It will now be seen why M. Arago elongated the jet and valve of De Caus's diagram into indefinitely extended pipes; evidently for the purpose of assimilating them to the pipes *f*, *g*, *h* and *k* of Savery's engine.

In the year 1705, a patent was granted to Newcomen, Cawley, and Savery, for a steam-engine which was invented by the two former, and which was the first that ever became extensively useful; for Savery's was limited to inconsiderable heights, unless the vessels were subjected to dangerous pressures, and it was moreover very expensive in its consumption of coal.

The machine of Newcomen and Cawley, which is still partially used where coals are cheap, consists of a boiler, *a*, from the upper part of which a pipe enters the bottom of the cylinder, *b*, in which works the piston, *c*, attached to the chain, *d*, whose upper end is fixed to one extremity of the beam, *e*, *e*. From the other end of the beam hangs the chain *f*, supporting

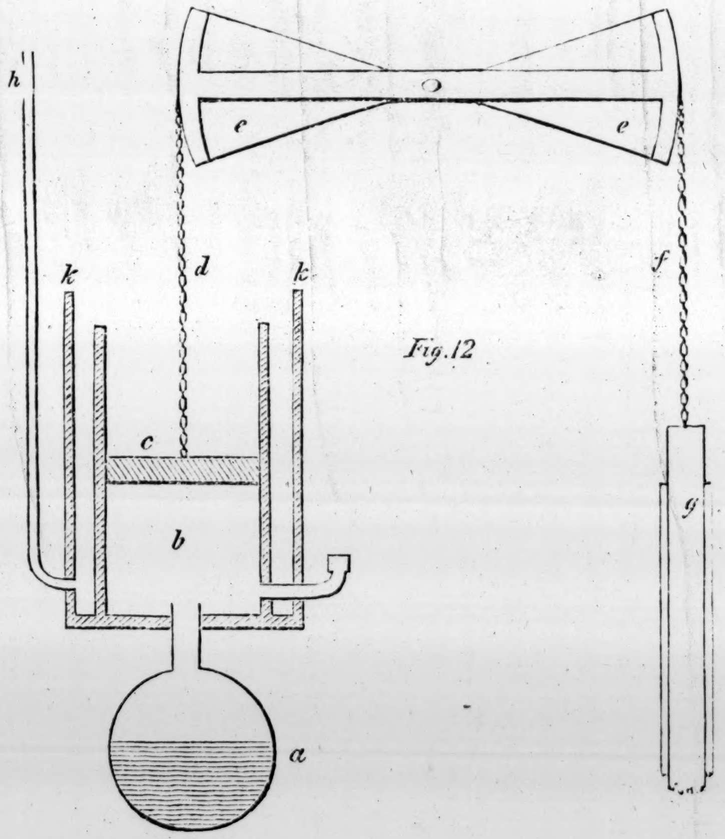


Fig. 12

the pump rod, *g*. A communication is opened between the boiler and the cylinder by moving a valve; the steam enters the cylinder, and the piston is raised by the weight of the pump rods. When the piston is at the top of the cylinder, the communication between the boiler and cylinder is closed, cold water is then suffered to flow through the pipe *h*, into an outer cylinder or case *k*, *k*, which surrounds the cylinder, *b*. The space between the two is then filled with cold water, which rapidly condenses the steam under the piston, produces a partial vacuum, and causes the descent of the piston with a quantity of power proportioned to its area, the weight of the atmosphere, and the perfection of the vacuum.

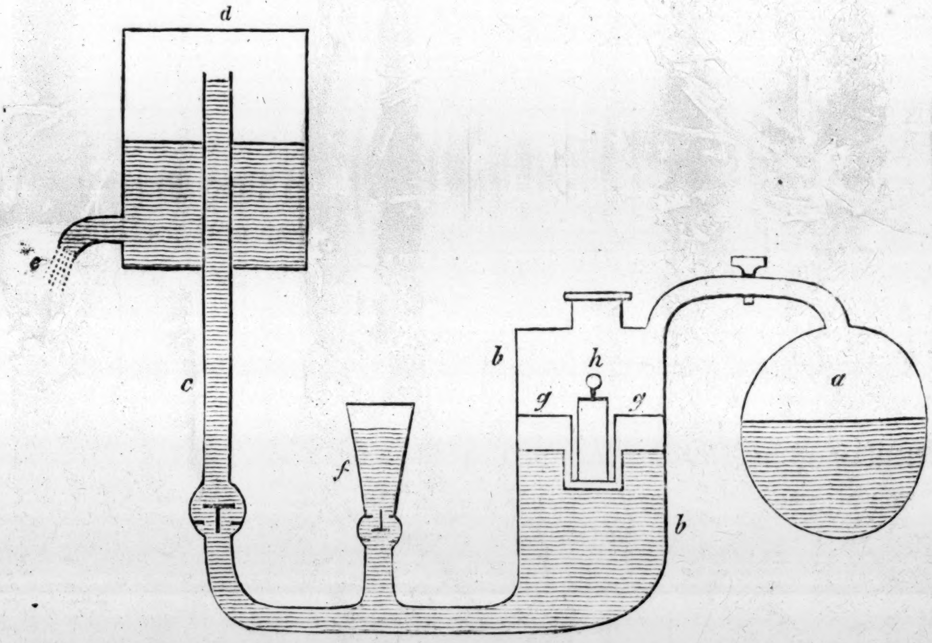
This engine, after it had been so far completed as above described, cost the patentees seven years' labour and experience before it became an economical machine for draining. Notwithstanding that, according to M. Arago, a " proper machine for draining" had been in existence ever since the work of Salomon de Caus, in 1615; and notwithstanding that Papin had, in 1690, published (he says) " the most clear and methodical description of the engine known at present by the name of the Atmospheric Steam-engine."

The engine of Newcomen, which M. Arago admits to be the first that rendered any considerable services to industry, he describes as he does Savery's, without a diagram. He says, that " excepting some very essential details, it is nothing but the machine proposed in 1690 and 1695, by Papin." Excepting some very essential details, it is merely the machine of Hero. M. Arago never gives the slightest hint that he has put Newcomen's valve and boiler to Guericke's cylinder, and ascribed the combination to Papin; he says, that " both Papin and Newcomen produced the condensation by cold, though the English mechanic adopted a better plan of cooling for a working machine than that which had been employed by Papin in his model;" and he proceeds, " The machine of Papin thus modified as to the manner of cooling the aqueous vapour, excited to the highest point the attention of the proprietors of mines."

In this last paragraph there is not merely a continued suppression of the fact, that the apparatus described as Papin's is not Papin's, but Newcomen's; there is a direct assertion that the two machines differed only as to the manner of cooling the vapour.

The "modification" which M. Arago thus misrepresents as to its extent, and which he accounts of so little consequence, made all the difference between exciting the attention of proprietors of mines, and exciting no attention at all. Papin's steam cylinder lay unnoticed and undisturbed, and apparently forgotten, not only by the world, but by Papin himself; for no sooner had Savery succeeded in perfecting and realizing his machine, than Papin abandons his cylinder, seizes upon Savery's engine, mutilates it, and publishes it, in 1707, as his own.

Fig. 13 describes this last and most palpable plagiarism, which Papin tells the Royal Society will so much increase the power of mankind, that he fears he shall be considered chimerical; he says further that it is much superior to the apparatus he had published some years before, and that he invented it without being aware of Savery's invention, which was (*he says*) not communicated to him till the year 1705. Papin omits the best part of Savery's apparatus, namely, the ascending pipes from the reservoir to the cold water vessels, and he claims this as an improvement. Thus he throws aside the only part of the contrivance which modern engineers have been able to use with any regard to economy.



The boiler, *a*, communicates with the cylinder, *b*, *b*, and the steam forces the water up the pipe *c*, into the vessel *d*, whence it escapes at *e*. The supply of cold water to be raised takes place at *f*. There is evidently not the slightest novelty in this machine so far; it is much beneath Savery's in every respect. It has, however, one novelty, from which a tolerably fair estimate of Papin's mechanical talents may be formed. The water in the cylinder, *b*, *b*, instead of being exposed to the steam, as in Savery's engine, is covered by a floating hollow piston of iron, *g*, *g*, supporting an iron heater, *h*, which is put in and taken out like the heater of a tea-urn. No two contrivances more absurd and wasteful than these were ever devised. The professed object of the hollow iron floater was to prevent the condensation of steam on the surface of the cold water. But even a hollow iron floater is a better conductor of heat than water, when the heat has to descend, and, therefore, the appendage is worse than useless. The iron heater to be put in and out at intervals is almost too ridiculous to notice, because, at the best, a certain quantity of heat applied in that manner could produce no greater effect than the same additional quantity applied to the boiler; while it is evident that the trouble of opening the cylinder, and exchanging the cold for the hot iron, together with the loss of heat in passing through the air, would more than destroy all that was gained. The whole contrivance is puerile in the extreme, and it is the work of Papin, of whom M. Arago says, "the man of genius is always misunderstood when he advances before his age."

The reader is now in possession of the stolen and disfigured child for which Papin deserted his former doubtful offspring, "the clear and methodical description of the Atmospheric Steam-engine;" and it will now be easy to estimate the respective claims of Egypt, Italy, France, and England, up to the year 1707.

But in fact the steam-engine was hardly yet invented. Newcomen's machine, though infinitely superior to every former contrivance, is a crude, imperfect, wasteful apparatus, in comparison with those which the following century brought forth. The engine up to this time is scarcely worth a dispute, even if it had been produced wholly by one man; but it is obviously the result of a succession of improvements so inconsiderable, that the whole of them do not exhibit a tenth part of the scientific and inventive resources which are displayed in the ameliorations effected by Watt alone. From the year 1705, the steam-engine is confessedly a British invention. For all that is refined and economical in the developement and application of the heat; for all that is ingenious in the machinery; for all that is vast in the power produced, and extensive in the purposes to which that power may be made subservient, it is notorious that the world is indebted to Great Britain. If, then, De Caus and Papin had done all that M. Arago so incorrectly ascribes to them, the great question would not be affected by it.

The reader who wishes to verify the statements here given, is referred to the "Acta Eruditorum Lipsiæ," from 1685 to 1700; to the 'Philosophical Transactions' for the same period; to Papin's work, printed in 1695 at Cassel, and entitled 'Recueil de diverses Pièces touchant quelques nouvelles Machines;' to Belidor's 'Architecture Hydraulique' in 1725; to 'Les Raisons des Forces Mouvantes,' by Salomon de Caus, of which an edition, printed in 1624, is in the British Museum; to the various editions of Hero's 'Spiritalia,' and especially to the translation in 1606, by Porta, entitled, 'I tre Libri Spiritalia,' of which a copy is in the British Museum.

If any objection be made to the language here employed, in regard to a person entitled to so much respect as M. Arago, it must be recollected that the paper of that gentleman is an indiscriminating attack on the motives of several of the most distinguished individuals of the United Kingdom, not all of whom have given as much cause for suspicion as any one of the misquotations in the paper of M. Arago.

In page 176 of that paper M. Arago says, "the greater part of the English authors," among whom he includes by name Dr. Robison and Dr. Lardner, "obstinately persist in quoting only one work of Papin, that of 1707, and refuse to take any notice of the more voluminous work of 1695."

The persons thus accused have at least the same excuse which M. Arago requires for neglecting Baptista Porta, with this additional circumstance in their favour, that they quote Papin's *latest*, and, according to his own account, his *best* contrivance, and that what they *do* quote, they quote *faithfully*.

They might well be forgiven for not supposing that the promulgator of the wretched apparatus of 1707 had invented the atmospheric engine in 1695; and it might have been expected that a paper making such grave charges should not have exhibited so many and such singular discrepancies between the representation and the truth, as have been here pointed out.

A. Ainger.

Some Account of the Order in which the Fossil Saurians were Discovered.

Sir,

Little notice was taken of the former discoveries mentioned in the Philosophical Transactions, of which, I believe, the Whitby coast furnished the first examples, when the *Rev. Mr. Peter Hawker* of Stroud, in 1812, set up a tent to trace some bones and a head, discovered in the blue lias at Weston, near Bath; and, following up the quarrymen's labours, succeeded in procuring the head and nearly all the vertebræ, with parts of the bones of the fins of a large animal, which, on his transmitting it to his museum at Stroud, caused much inquiry, and a desire in others to procure specimens. Numberless fragments were excavated at Bath of jaws and teeth very perfect, of which about that time the author procured some fine samples; and *Mr. Hawker's* crocodile, as it was then called, induced *Mr. Parkinson* to get a drawing of it; but the artist he employed omitted to notice a peculiarity in the scales that surround the eye, which the writer of this notice afterwards discovered, and engraved to give away. Soon after *Mr. James Johnson* of Bristol, in 1813, raised an enormous head from low-water mark at Lyme, and brought it to Bristol at the expense of twenty pounds; it weighed, I believe, near a ton, and was 2 feet 6 inches broad, by 2 feet 10 inches long, and 11 inches thick. There are two large cavities on the sides of the frontal bone, and the sockets of the eyes are 14½ inches long, by 7 inches wide; a few of the scales of the eyes were seen, and were near an inch in diameter—the sockets being oblong, not oval, as were those of Mr. Peter Hawker's specimen. About the same period *Mr.*

Day, of Henton, got a very large head imperfect, but several of the teeth in their places. Next, *Mr. Brakenridge*, of Bristol, acquired from the Theynsham quarries a very large and singular head, with high frontal bones, and a circular eye, also having plates very distinct, and a considerable number of teeth: this was also discovered about 1813. *Mary Anning*, of Lyme in Dorsetshire, to whose industry and skill we owe nearly all the fine specimens since found, next sold to Mr. Bullock, a head about 5½ feet long, with fifty teeth in each jaw, the eyes also defended by sixteen scales; which having been purchased of him by the British Museum for forty-seven pounds, *Mr. Kenig* cleared away the superfluous limestone, and discovered the nostrils. All these were ichthyosauri. *Mr. Johnson* next procured a head of the Gangetic crocodile from Parbee island; and, in 1819, at Whitby, in the alum shale, half a mile from the entrance of Whitby harbour, another was disclosed of this tribe; it was 3 feet long, and had several teeth resembling *tenui-rostris*, and several of the vertebræ adhering: this has been engraved.

In 1818, Mr. *Morgan*, of Bristol, from the schists, at low water, at Watchet, in Somersetshire, extracted nearly the whole animal with the fins adhering, ribs, and entire tail. This is the only one found there, and was purchased by the *Surgeons' Company* in London for 25*l*. The Geological Society also procured one nearly entire, of *Mary Anning*, of Lyme; Colonel *Birch* had another; M. *Delabèche* a fourth, all nearly perfect. After which the *plethiosaurus*, now in the possession of the *Duke of Buckingham*, and for which he gave Miss Anning above 100*l*., was brought to light by her patience and assiduity; and, lastly, she raised the *tenui-rostris*, a new species that has been acquired of her for 50*l*. by the *Bristol Institution;* since that, others even more perfect of the *ichthyosaurus* have come to light; but the most perfect of any, she raised at considerable expense, in 1824, and I believe still possesses it; having attained it by lifting the bed of schistous limestone inwards to the coast, and removing it piece by piece, and even turning it to clean it and shew the best surface. Since that period little has turned up, until 1826, when at *Whitby*, in the schist, they are said to have discovered a true *crocodile* fossil, which is now in the museum there, and is published by *Dr. Young*, their secretary, in his second edition of his Survey of the Yorkshire Coast.

In *Brook's Journal*, vol. xxv. page 101, is an account of one of these animals, found eighty feet from the surface of the earth in 1810, near Stratford-on-Avon. He called it a crocodile. *Cuvier* says one was found at *Maestrich quarries*, and conjectures it to be an intermediate genus between animals of the lizard tribe which have long and forked tongues, and those that have a short tongue, and the palate armed with teeth: the skeleton was twenty-four feet long, the head one-sixth of its length: he thought then it inhabited the ocean.—See *Jameson's* Essays. Fragments of heads have been found in the *Vincentine* in Italy, in pyritical blue clay limestone, also at the bottom of the cliffs at *Honfleur*, in France, and *Havre;* at *Alençon* was found one also which *Cuvier* considered as a lost species of *crocodile;* at Newark, in Nottinghamshire, *Dr. Stukeley* found what he called a lost species of crocodile. Professor *Jameson* says all these fossil remains of oviparous quadrupeds belong to very old flötz strata; but they seem to me to be more related to the porpoise and dolphin than the lizard, which they only resemble in the jaw bones, their fins (ridiculously enough called paddles) being very nearly resembling those of the porpoise, as does the general character and attitude. At *Colonel Birch's sale**, at Bullock's Museum, his specimens procured him a great price, but the principal one, which was that engraved for the *Royal Society*, it was said he bought in at 120*l*. Now much more perfect ones are offered by *Mary Anning* for 50*l*., one indeed quite perfect. A work containing good engravings of all the varieties now in England would be a very desirable acquisition to naturalists, and would do credit to the country, if accompanied by a good history of the whole tribe from true documents; but such a work should be a national undertaking: for, under the present state of discouragement from the expense of printing, it would ruin any individual. Even our rarest collections of geological specimens will not pay the publishers; and unless government, through parliament, afford their assistance, the French nation *will and must take the lead of us in this branch of natural history.* To *Cuvier*, we owe the osteological key to those discoveries, which *Sir E. Home, Mr. Coneybeare*, and *Professor Buckland* have used freely to unlock the character of the specimens found at Lyme and elsewhere; but we ought ever to remember, that the world would to this day have remained ignorant of the treasures England possessed, but for the patient labours of three female pioneers in this service, viz. *Mary Anning*, a dealer, *Miss Congrieve*, and *Miss Philpots*, residents, who for years had been collecting and preserving these bodies from the wreck of the coast; the two last without any other view than the gratification of a laudable curiosity, and who, with unequalled liberality, communicated their collections to every man of science that visited the place; and it is to liberal minds like theirs and *Miss Bennet's*, of Wiltshire, that we owe the first rescuing these natural gems from the spoilers, and the consequent credit in which this country, on this score, is held by surrounding nations. They, and a few others, gathered the materials of this fabric raised to fame, and are entitled to a full share of the honours reaped by those who, without their aid, could never have brought them before the world, yet, some of whom, with a vanity that *greatly impedes scientific pursuits*, affix their own insignificant names to every little shell they find, or purchase of some poor quarrier on the road side; so that now we have not less than *twenty-three fossil ammonites*, that have little or no other description to know them by than the family names of the *supposed* first finders!

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* The professed object of which, he said, was to provide for Miss Anning, of Lyme.

In almost every other department of fossil conchology, indeed, the same abuse prevails, and this tickle-toby method has now been carried too far. To the discoverers of such systems as those of *Werner*, or *Hutton*, or *Cuvier*, there can be no objection to attach their names; it is both for their honour and our own convenience; but in scientific pursuits, this scramble for notoriety disgraces the appropriators of such fragile monuments, for all must be demolished before we can know, by a marked distinction, what shell or fossil remain we are possessed of, or talking about, so as to come to a knowledge of the stratum to which it belongs. The worst is, that this greediness of fame infects even public societies, and they would cancel, if possible, the discoveries of private individuals unconnected with them, or veil them by neglecting them in their general reports, until such time as the leading members of their own institution are prepared to amalgamate them into their papers as previously known to them; and this system of exclusiveness is not only unhandsome and unjust, but prejudicial to the discovery of truth in science, as it checks the ardour of many solitary labourers in every branch of study, and so far impedes the march of science. Next to an affected nomenclature, is the desire some men entertain unnecessarily to change its language, in the hope that no one will afterwards be so well able to understand it as themselves. Such are always the resources of little minds. Not such the ideas of the great literati of Italy, who, wishing to improve their country and language, used, in preference, the commonest expressions and simplest construction of sentences, as the only last and best method of conveying intelligence to the general mind. Such were the noble views of our immortal *Addison;* and had we not degenerated from his especial example to this day, we should have not only been able to convey our ideas, without refraction through the medium of fashion, to each other, and teach the sciences to common capacities as easily as religion, but have been perhaps the parents of much useful discovery to the whole world.

I am, &c.

G. CUMBERLAND.

Bristol, 2d June, 1829.

Proceedings of the Royal Institution.

The weekly meetings of the members of the Royal Institution were resumed for the season, on Friday, the 23d of January. Upon the table in the Library was a bust of the late Daniel Moore, Esq., executed in marble by Sievier, for the Royal Institution, to which Mr. Moore was a most liberal benefactor. Various Hindu mythological paintings and drawings were exhibited, relating to the history of Crishna, the Apollo of the Hindus. A variety of bows and arrows, illustrative of the history of the weapons of various nations, were upon the table; as also some remarkably fine castings in bronze, and various novelties in literature and the fine arts.

In the Theatre, Mr. Brande gave an account of the present state of the supply of water to the metropolis. In bringing this subject before the members of the Royal Institution, he said he was actuated by two motives: the one, that of calling and keeping up attention to a subject of the utmost importance to the comforts and health of the inhabitants of London; and the other, that of divesting it of certain prejudices which have too often attended the discussion, and of shewing the possibility and plausibility of effecting such improvements in respect to it, as should tend to meet all solid objections. He observed that there were numerous points of interest involved in the general consideration of this question, and that the quality of the water, as affecting health, was a principal one—it must be in itself salubrious, or it may produce direct mischief, and there must be abundance of it to wash away those accumulations of filth, which would otherwise generate *Malaria*, and various diseases. In respect to salubrity, some waters are so evidently contaminated, as to be unfit for general purposes, and which, though used from necessity in certain localities, could not be tolerated for the supply of a town; of these it was unnecessary to speak. Others, although not obviously unpalatable, might be injurious, from certain contamination of organic substances in peculiar states of decay—this seems formrely to have been the case with the water of the Neva—perhaps also with that of the Seine—the subject is of importance, because such contaminated waters, although containing very minute and almost undetectable quantities of noxious matter, may slowly impair the constitution, and render it susceptible of peculiar disorders. The principal points referable to abundance of water as a source of health, were then touched upon, the influence of extensive drainage, and properly constructed sewers, of street-watering, and of allowing large quantities of water to run to waste: these several points were enlarged upon, and their general influence upon the cleanliness and sweetness of London shewn, notwithstanding the great evil the town has to cope with in the torrents of smoke with which it is infested, and which evil promises to be aggravated by the utter neglect of all attempt at its consumption, and by the recent invention of circular polished flues, which throw all the soot into the air, instead of allowing, as in common constructed chimnies, much to be deposited in the flues. To make persons more content with the present supply of water, Mr. Brande contrasted the quantity and quality of that supplied to London with the supplies of other large towns on the continent, where it is carried in driblets from house to house, where there are no sewers, and where all kinds of discomforts arising from these causes are severely felt. How far the decoration and ornament of a town might be connected with its supply of water, were next considered. The unostentatious abundance in London was contrasted with the ostentatious penury of Paris: a very little water easily made a great show in a well-constructed fountain, and in these respects we are apt to be much misled by appearance. At the same time, ornament is not to be depreciated where it may be so admirably associated with utility; the grand fountains of Rome were cited as instances, and regret expressed at the utter inattention to decorative water-works in our new buildings. It was well understood that the excellent opportunity now offered of effecting something good in this line, by the reconstruction of the Chelsea Company's reservoir in the Green Park, was to be neglected.

From these general considerations Mr. Brande proceeded to such details as affected London in particular, noticing especially the extent of the metropolis, and the peculiar mode of supply—many hundred miles of pipes of different dimensions were required, capable of sustaining the pressure of one hundred feet of water; for this purpose iron was a necessary requisite,—great capital was necessarily expended in machinery and utensils fitted for this supply, and for throwing into the town a daily supply of thirty millions of gallons, which is the average consumption.

The sources whence the water for the supply of London was obtained, were next particularized, and the relative quantities drawn from the New River and the river Thames were estimated. In further illustration of this subject, the following Table was shewn, specifying the name of the Company, the quantity of supply, the number of tenants supplied, and the number of engines employed.

COMPANIES.	Gallons supplied Daily.	Cubic Feet supplied Daily.	Tenants supplied.	Number of Engines.		Aggregate Horse Power.
New River .	13,000,000	2,000,000	67,000	3	60 60 100	220
East London	6,000,000	950,000	42,000	4	40 46 70 90	240
West Middlesex	2,250,000	360,000	15,000	3	70 70 105	245
Chelsea .	1,760,000	282,000	12,400	2	60 70	130
Grand Junction	2,800,000	450,000	7700	3	100 100 70	270
Lambeth .	1,244,000	200,000	16,000	2	36 80	116
Vauxhall .	1,000,000	160 000	10,000	2	45 20	65
Southwark .	720,000	115,000	7000	2	42 20	62
8	28,774,000	4,517,000	177,100	21		1342

Mr. Brande said, that this Table furnished abundant matter for discussion and reflection ; but the point to which he should now principally call the attention of his audience was, that the greater part of the Thames' supply was taken from the most objectionable parts of the river ; that is, from those parts which immediately receive the drainage of London : and although there is certainly a constant and considerable admixture of fresh water supplied by the descending river, and partly, perhaps, by the influence of the tides, there could, he said, be no doubt as to the mass of water being materially affected by the impurities thrown into it ; and as these have greatly increased within the last twenty or thirty years, so there has been a proportionately increasing deterioration during that period. Mr. Brande then went into the proofs of this deterioration, and detailed its various causes, principally from the evidence laid before the Commissioners, and published in the Parliamentary Reports. He observed that it was surprising, considering the abundant and prolific sources of all kinds of impurities to be found in the district alluded to, that the water was not more polluted; and that at times the supply was tolerably unobjectionable, especially when it had been subjected to filtration: whence we should infer that the chief evils arise from matters mechanically suspended in, and not chemically combined with, the water. He observed, however, that this remark was not made to justify the present sources of supply: for, even if the water were quite unobjectionable, the very circumstance of its being drawn from a part of the stream polluted by the contaminations of a million of human beings, to say nothing of the thousands of other animals and other sources of mischief, should awaken suspicion, induce us to suspect its cleanliness, and most undoubtedly to discontinue the supply from such sources, unless impelled to retain it by the sternest necessity, by the utter impossibility of getting water elsewhere, which fortunately is not the case. The question as to the purity of the water with which London is supplied, involves two important points—its salubrity—its composition. In reference to the first, even a professed chemist must allow, that water *may* be insalubrious, though chemical analysis cannot detect the cause of the evil; air may be eminently so, and why therefore not water? It was by no means meant to imply that the water *is* unwholesome, because all general facts were against such a conclusion, but merely to insinuate that we should not be thrown off our guard. In respect to the actual analysis of the Thames water, the following is a comparison of the purest and impurest specimens —10,000 parts leave, by evaporation,

	Purest.		Impurest.
Carbonate of lime	1.53	..	1.55
Sulphate of lime	0.15	..	0.12
Common salt and muriate of magnesia	0.20	..	0.23
	1.88	..	1.90
Organic matter	0.07	..	2.02
	1.95	..	3.92

By reference to the above, and other data, laid before the commissioners by Dr. Bostock, it appears that the extreme impurity of much of the water with which London is supplied, is referable to matters mechanically suspended in it, and that it consequently may be much improved by keeping it for a due time in tanks or reservoirs properly constructed to admit of the subsidence of the impurities, and the drawing off of the clear portion. Mr. Brande then described the efficacy of this operation as conducted on the works of the Grand Junction Company at Paddington, and adverted to the signal improvement which had thus been effected in their supplies. He next described the process of filtration as applicable to very large quantities of water, and gave an account of the works lately carried into effect for this purpose by the Chelsea Company, and shewed a model of their arrangements for the purpose, for which, as well as for the following particulars, he was indebted to Mr. Simpson, the engineer to that Company:—

"The pond which contains the filter bed is forty-four feet square at top, and made with sloped banks, the bottom being twenty-six feet square; it is six feet deep; the mode of forming the bed was as follows:—After the pond was made water tight, with a drain through the bank to the well, the bottom was covered with coarse gravel, in which drains were built without any cement between the joints of the bricks; they were covered with coarse gravel, and then with finer gravel, with coarse sand and finer sand, until the strata of gravel and sand were each two feet thick, both gravel and sand having been selected with care, and well washed. The reservoirs were each thirty-two feet square at top, twenty feet at bottom, and four feet deep; the low water line of them being level with the high water line of the filter bed; the reservoirs were worked alternately on to the filter bed, and it was regulated to filter 12,000 cubic feet of water every twenty-four hours; and the water was remarkably pure and limpid after it had passed the bed. The silt which was stopped on the bed was regularly cleaned off with a small portion of the sand every fourteen days; the principle of the action depends upon the strata of filtering material being finest at the top, the interstices being more minute in the fine sand than the strata below, and the silt, as its progress is arrested, (while the water passes from it) renders the interstices between the particles of sand still more minute, and the bed generally produces better water when it is pretty well covered with silt than at any other time. The silt has never been found to penetrate into the sand more than three inches, the greatest portion always being stopped within the top half inch of the sand, and in cleaning the silt off, it has never been found necessary to scrape any more of the sand off with the silt than the first half inch depth, and sometimes only half that depth was removed. The small air pipes from the drains are to prevent injury to them or the filtering materials by condensation or otherwise.

"The large filter bed at Chelsea is a surface of nearly one acre, and is constructed precisely on the same principle as the experimental bed, the details of forming and working it being greatly improved and adapted to the enlarged scale. The perfecting this bed has been a work which required the greatest attention and perseverance; it was set to work, and supplied the Chelsea Company's district for the first time on the 14th of January, 1829, and is of sufficient capacity to filter 500,000 cubic feet of water every twenty-four hours: it is working with the greatest success during this inclement season, and although the water on the bed is this day covered with ice five inches thick, it does not impede the filtering process.

"This filter bed was made after the engineer had seen several similar works upon a small scale in England and Scotland, which have been at work some years, and he has combined in it the several advantages he observed in the filter beds he examined.

"It is proposed to follow up the improvement by extending the suction pipe up the river, and pumping the water (during the last two hours of ebb tide only) into small reservoirs constructed close to the filter bed, and the water will be filtered as it is required for the supply of the town, and the main pipes will be so constructed that none but filtered water can be pumped into them."

It will be seen by Mr. Simpson's evidence before the commissioners appointed by his Majesty to inquire into the state of the supply of water to the metropolis, that the water, by filtration, could be rendered perfectly clear, excepting during extraordinary land floods, when upon a close inspection he had perceived a slight loamy colour in the water after it was filtered, which was scarcely discernible in a glass tumbler. Mr. Simpson now remarks that the water in the large filter bed is much better than it ever was in the experimental bed.

After some general remarks upon the improvement of the water by deposition and by filtration, Mr. Brande remarked that still as far as the Thames is concerned, the great evil remains of employing contaminated water; other sources of supply, therefore, are to be looked for; these are three-fold—springs or wells—other rivers and streams—the Thames, so high as to be unobjectionable from acquired impurities. The latter source being abundant, unobjectionable, and as easily attainable as any other stream, appeared that to be preferred, and the various suggestions as to the best mode of carrying such plans into effect were briefly detailed, especially those proposed by Dr. Kerrison, Mr. Philip Taylor, and Mr. Mills, the principal particulars respecting which are detailed in the parliamentary report of the commissioners.

Friday, January 30.

Signor Nobili presented specimens of his metallo-chromes. They consist of plates of steel, with a series of intense iridescent colours ornamentally developed upon them. The effect is produced by a peculiar application of the power of the voltaic pile.

Mr. Cuthbert's improved telescope and stand, and also specimens of meteoric iron, and of crystals of titanium were exhibited.

In the lecture-room Mr. Gilbert T. Burnett discoursed on the subject of vegetable metamorphosis. After some prefatory remarks on the popular and classic acceptations of the term, and allusions to "those extraordinary transformations which suit so well the fable of the poet, but require nor name nor notice in the adversaria of the philosopher," he continued: "By the metamorphosis of vegetables I would not, however, as some naturalists have done, include merely those regular transitions, those ordinary changes which, in succeeding seasons and successive stages of existence, plants as well as animals ordinarily undergo; which changes are but developements of the individual: as when in the one instance the embryo becomes a fœtus, the fœtus an infant, the infant a child, the child an adult, and the adult wanes through decrepitude and age; or, as in the other, when the germ becomes a seed, the seed a herb, and the herb a perfect plant, which, after passing through its flower and fruit bearing stages, decays and dies; which epochs mark the changes that all must undergo, but which are in none more evident than in insects and in plants; for the egg, the larva, the chrysalis, and image of the one, are analogous to the seed, the herb, the bud, and blossom of the other. Indeed, it hath been well observed that 'the flower bursts forth from its larva, the herb, naked and perfect like a butterfly from its chrysalis, winged with its corol, wing-sheathed with its calyx,' and furnished with stamina and pistils for the fertilization and perfection of the seed. These natural and ordinary revolutions are curious and important; they cannot pass entirely unnoticed, yet the notice must be brief, as our more especial purpose now will be to indicate those preternatural metamorphoses, occurring either spontaneously, or produced by art, in which the ordinary developement of the whole or certain parts of plants is affected either by perversion, augmentation, or diminution; some of which changes are hurtful, and some highly conducive to the comforts and conveniences of man. Hence to the question, what mean we by the metamorphosis of vegetables? The answer may be shortly stated thus:—

"Metamorphosis refers to all the notable changes which organic beings more or less ostensibly exhibit in passing through their several stages of existence, and these are threefold. 1st, the regular or ordinary developement of parts, *i. e.* their transitional metamorphosis; 2d, the irregular or compensating developement, *i. e.* their metastatic metamorphosis; 3d, the extraordinary or anomalous, often called the monstrous, developement, *i. e.* their casual metamorphosis."

Among the various instances adduced of transitional metamorphosis, the ivy formed the best marked example; and with the additional characteristics noticed by the lecturer the description seems correct. "The ivy, in its infant or very young state, hath stalks trailing upon the ground, and protruding rootlets throughout their whole extent; its leaves are spear-shaped, and it bears neither flowers nor fruit; this is the hedera humilis, the variety which Bauhine describes as 'humi repens, ivy creeping upon the ground. The same plant, when more advanced, quits the ground, and climbs on walls and trees, its rootlets becoming holdfasts only; its leaves are generally three or five lobed, and it still is barren: this is the hedera major sterilis of Bauhine, the greater barren ivy. In its next, or more mature state, it disdains all props, and rising by its own strength above the walls on which it grew, occasionally puts on the appearance of a tree; in this the flower of its age the branches are smooth, devoid both of radicles and holdfasts; and it is loaded with blossoms and with fruit; the lobulations of the leaves are likewise less: this is the hedera arborea of Bauhine, the war poet's ivy. But when old, the ivy again becomes barren, again the suckers appear upon the stem, and the leaves are no longer lobed, but egg-shaped: this is the Bacchanalian ivy." Yet, as Rose observes, daily experience proves, that not the ivy alone, but all plants pass through similar stages, and undergo the same fate. While germinating, they are fed from food provided in the seed by the parent plant; in their infancy they are weak, feeble, barren; afterwards they wanton in luxuriant foliage, being, as it were, in the joyous spring of life; next, in the flower of their age they are decked with beauteous and comely blossoms, and in maturer life they are fertile but less showy, producing fruit and seed: in autumn they wane, become dry, barren, hoary; first one part decays, then another, till the whole plant is destroyed, and having run through its several stages of existence, at last it dies.

The change in size produced by growth must not be forgotten; some plants are always diminutive, whilst to others a vast bulk is common. In these latter the transitions are most notorious, and the change is most extraordinary. See the embryo in the acorn, so small as seldom to be noticed, and weighing not a grain, becomes the gigantic monarch of our woods, affording from ten to twenty tons of timber. Behold this embryo, so soft and tender as scarcely to resist the finger's pressure, becomes the "unwedgeable, the gnarled oak!" &c. &c.

The transitional metamorphosis of certain parts of plants forms, (said Mr. B.) a subject of curious speculation, and each stage of developement in the more perfect tribes would seem to be permanent types in the lower grades: thus the foliage which is absent in aphyllous plants, in stapelia and cactus is latent in the stem, the simple culms of rushes, and linear leaves of grass, resemble the unexpanded and flattened petioles of other tribes; the successive scales of an opening bud are allied to the ambiguous foliage of the moss, and the gradation from a hardened squama to a perfect leaf may be traced in most, but in none more beautifully than in horse-chesnut gems; where the perules become finer and finer, and the true leaf, at first crowning the scales as a point, becomes more and more developed, while the perule, under the form of stipule, becomes an expanded petiole to the earlier leaves. In some plants, as heracleum, rheum, &c., the hollowed petiole forms a covering to the buds, and it is subsequently a stipule. That organ which in iris must be deemed a leaf, in narcissus becomes a bracte; in arum, a spathe; in lily, at first a calyx, though afterwards corolla; and in calla, it successively is each: and thus, cultivation in many, as in the tulip, will develope on the naked stem indifferently, a leaf, a bracte, or a petal. The winged filaments of ornithogalum and the pistils of iris show their affinity with petals, and the corolla of the former is posteriorly a calyx also. In double flowers the transitions may be traced in every stage. Ranunculus aquatilis affords an admirable instance of the conversion of water into land leaves. I have a specimen in which one half of the same leaf is lobed and the other fibrous, as one half was above and the other below the water.

Of the irregular, or metastatic metamorphosis, it was observed that it is a law in physiology that certain organs have certain functions to fulfil; and for the due performance of those functions, they have an appropriate and peculiar structure; *e. g.* in plants the roots usually absorb nourishment and have a structure fitted for that purpose; the stem usually bears flowers, which usually produce seeds, &c. &c. Now when the stalk or the leaves change their form, and protrude fibres that absorb nourishment, taking upon them the structure and the offices of roots; and when the flowers which usually produce seeds, produce buds, or bulbs, or roots; or, on the contrary, the roots themselves develope buds; these changes and the like are irregular, or metastatic metamorphoses. Again, when buds, which usually produce leaves and flowers, produce roots, and such as usually produce roots evolve foliage and blossoms, these changes and the like are also denominated metamorphoses by metastasis. Of these and other sections of metastatic metamorphoses, as the acclimation of plants, many examples were given—let a few suffice. The evolution of rootlets on the stems frequently occurs on old trees, when sufficient nourishment cannot ascend through the decayed and injured trunk. "A lilac tree (said the lecturer) grew for many years against a wall in the yard behind a house in Hart-street, Bloomsbury; the greater part of the tree being dead, on attempting to remove it, one branch was found firmly rooted in the wall some feet from the ground. All the rest of the tree was cut away, and this scion lived and grew for several years in the curious situation it had fixed on for itself, its only nourishment being derived from the wall through the rootlets protruded from the stem. I have in wet seasons frequently detected rootlets protruding from the axillæ of the leaves, and in some plants, as the œnanthe crocata, to such an extent, that the lower part of the stem was very considerably smaller than the middle. Occasionally in old trees, which are rotten at the heart, the branches will send down roots through this ligneous soil, thus forming several young and healthy conduits within the old decaying case. In some species this rooting from the branches is common, as in the mangrove, which will form a continuous forest from a single tree; and the banyan likewise has been known to have between 300 and 400 of these secondary stems. I am informed that at Revel-gong there is one that covers upwards of five acres of ground, and this is by no means of the first magnitude."

This power of protruding rootlets from the stem, which more or less prevails in most plants, is often taken advantage of, for the propagation of small and early bearing trees: in fact, the success of layers, slips, pipes, &c., all depend upon this peculiar power. The complete inversion of a tree, as of a willow, gives an excellent example of the metamorphosis of buds, for the roots of the trees will become covered with leaves and flowers; and the branches planted in the earth will develope roots. In pear, apple, and other fruit trees, in which the buds which will produce leaves are, by a practised eye, easily distinguished from those which will produce flowers and fruit, the latter being called bearing buds, the conversion of the one into the other is continually effected by art. "A friend of mine had three pear trees growing against a wall, having an eastern aspect, and a good deal shaded; these trees year after year were luxuriant as to leaves, but never blossomed; the groom having tethered a horse to one of these, the animal gnawed it three parts round, denuding it of bark for the space of several inches; that tree, and that alone, the succeeding summer bore fruit; and a practice similar to this is frequently resorted to for the conversion of leaf into flower buds: for so easily can they be distinguished, that I am told it is no uncommon thing in the cyder counties for the crop of apples to be bargained for and sold merely from the promise of the buds." The physiology of this conversion Mr. B. attempted to explain, by the hypothesis that every bud was a distinct individual including, potentially at least, roots, leaves and flowers, but that in certain circumstances only were the several organs developed; and that when a tree is inverted, and its roots evolve leaves, and its branches roots, it is not a mutation of leaves into roots, but the developement of that part which, under other circumstances, would have been obsolete, and in the obliteration of that which would have been developed; and the same of the leaf and flower buds. As examples of the metamorphosis of seeds into gems, specimens of proliferous flowers were shewn; and the separation of buds, as in lilium bulbif., &c. was instanced as an approximation of these to the character of seeds. An example was shewn of this spontaneous separation of a bud or tumor in a cedar tree.

Of the acclimation of plants many instances were given, and specimens on the table of plants which, having migrated from various countries, are naturalized in very different latitudes; to this it would seem that we owe the chief of our vegetables, fruits, and flowers.

Of the extraordinary or fortuitous developement, better called the casual metamorphosis, time allowed but one section to be referred to, viz., the first or simplest grade relating to excess of growth, either in the whole or particular parts of certain plants, the metamorphoses occurring in monstrous, mule, and mongrel plants being reserved for another opportunity.

Plants in their wild state, observes Wildenow, remain pretty constant in their appearance; but no sooner are they domesticated, than, like animals, they begin to change their size, their colour, and their taste.

Of increase in size many instances were shewn; *e. g.* leaves of potentilla anserina upwards of two feet long; of horse-chesnut, twenty-two inches across, &c. &c. The grass called fiorin (agrostis stolonifera) in barren soils and on the sea-shore seldom exceeds three or four inches in height, but in fertile soils it is so greatly changed as to produce from 16,000 to 18,000lbs. weight of hay per acre, and two acres and a half have yielded ten tons of hay in one year; indeed, Dr. Maton collected specimens about seven feet long at Orcheston, and likewise, at the same place, found poa trivialis nearly ten feet long. The ordinary height of meadow clover may be about ten or twelves inches, but it sometimes reaches four feet, &c. &c. The chief object of the lecturer seemed to be to shew the complete dominion that man possessed over the fruits of the field, and that so changed had they become that many might, with some latitude of expression, be said to be made by man. Thus, wheat is no where to be found wild, or, if found, as hath been asserted, in Thibet, it is so altered, that the most accomplished botanists doubt the legitimacy of the descent, so far superior is the offspring to the reputed stock. Who, in the wretched sloe, would recognise the parent of our most luscious plums? or think our most luxuriant apples the offspring of the austere and verjuice crab? The cat's head and some Normandy apples will singly weigh from eight or ten ounces to one pound, which is often as much as the entire crop of the ancestral crab. Of this increase in produce, Withering records an excellent example. At Home Lacy, says this author, there grows a pear tree, which, from successive layers, hath spread over nearly a quarter of an acre, and has yielded for many years sufficient fruit to form annually from twelve to sixteen hogsheads of perry. Who, in the almond's rough and leathery coat, would recognise the rudiments of the luscious peach? yet not unfrequently this acquired luxuriance is checked, and the original state of almond is found on the same tree, nay, even on the same branch with the factitious peach; occasionally one half of the same fruit retains the acquired character of peach, while the other degenerates to the almond state. Similar changes are also sometimes met with in apples, one half being small and austere as the parent crab, the other remaining an undegenerated nonsuch. "In our garden at Laleham, a few years ago, a single peach tree produced the amazing crop of 130 dozen fine peaches, besides a vast quantity thinned away in the early part of the season."

Specimens were shewn of many garden vegetables in their uncultivated state, that they might be contrasted with such as are now brought to the table; *e. g.* the apium graveolens, thin, dark green, and very acrid, would not be known as our common celery, when, by cultivation, the leaf stalks have grown upwards of five feet long, and two inches in diameter. Petioles of rhubarb have been cut nearly three feet long by six inches in circumference; and twelve, when trimmed for the market, weighed upwards of a quarter of a hundred. Specimens of the wild cabbage were shewn, not weighing, leaves, flowers, and all, quite an ounce; now these contrast finely with the offspring, cauliflower, the blossoms of which will weigh several pounds, and with the large cabbages, the leaves of which have been known to exceed sixty pounds in weight; an account of one has been published which measured five feet round.

Similar metamorphoses were illustrated by wild specimens on the table of the lecturer,—asparagus, seacale, parsnip, carrot, &c. The latter roots are small and woody; but when cultivated increase so much in size and succulence, that they have been known between two and three feet long by more than one foot round, and weighing upwards of four pounds; the potatoe, also, when growing wild in Chili, and in unfavourable situations, is small as a common marble, and very bitter; but when in proper soil and cultivated, a single tuber will weigh from one to two, or even, as hath been stated, nearly three pounds.

The lecturer shortly alluded to the absurd doctrine at one time prevalent, of the conversion of one plant into another; as of *wheat into cow-wheat*, darnel into oats, &c. &c. and shewed that although cultivation might improve, and subsequent neglect deteriorate, the degeneration was only a return to that state from which the variety had been fortuitously removed. He concluded by observing, "Doubtless, many of the circumstances to which I have referred already were familiar to many of my audience. Many of the metamorphoses on which I have had to comment are passing continually before your eyes; many of the details of these changes are daily sounding in your ears: but they are not the less curious, because they are common, nor the less worthy notice because they can by all be known; their certainty rather than their mystery, their frequency rather than their rarity to me, are gages of their importance; though some persons may not impossibly feel as much astonished, that changes so common and familiar should be dignified by the term metamorphosis, as Molière's Bourgeois Gentilhomme was astonished to find out, that he had all his life, although unconsciously, been talking prose."

February 6th.

In the library were a series of cosmospheres and cosmospherically mounted globes, invented by Major Muller, who was present to explain their principle. M. Nobili's exceedingly sensible galvanometer was also on the table, with its accompanying portable apparatus, by which the testing powers of the instrument are applied. Mr. Nobili was present to explain it.

In the theatre, Mr. Green delivered a lecture on the study of ancient coins, which he has since published.

February 13th.

A variety of interesting objects of art and antiquity were upon the library table.

In the lecture-room Mr. Faraday explained Mr. Brown's discovery of apparently active molecules in bodies.

A brief account of Mr. Brown's results has already been given in our Journal*. Mr. Faraday, by means of drawings and specimens, gradually developed the path by which Mr. Brown was led to the singular conclusions at which he has arrived; the care that he took to test the appearances he met with; and the state of his opinions at present, which in public have been much mistaken.

First engaged in examining pollen and the particles from its grains, Mr. Brown found that all solid matter insoluble in water, with very few, if any exceptions, when reduced to powder and suspended in water, gave particles which exhibited, under the simple microscope, peculiar motions. One kind of motion was progressive, the particle advancing forwards more or less rapidly, and changing places with the neighbouring particle; another kind was a vibratory action, in which the particle seemed rapidly to oscillate backward and forward about an axis nearly vertical; at other times, the particle would advance, then recede, then advance; and in that manner oscillate, moving at such times over a space equal to a half or a whole diameter, two or three complete vibrations occurring in a second. With particular substances, linear arrangements of particles were frequently seen to move, the appearance then resembling a twisting or vermicular motion. The molecules usually appeared of a rounded form, and had been estimated by Mr. Brown and others, as being from the $\frac{1}{15000\text{th}}$ to the $\frac{1}{20000\text{th}}$ of an inch in diameter, but in later observations minute angular particles were distinctly seen in motion.

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* New Series, vol. iv. p. 234.

Mr. Faraday stated that Mr. Brown had endeavoured in every possible way to justify by experiment the reference of these motions to some known cause, but had failed in doing so. Evaporation had been prevented by inclosing the fluid and particles, sometimes by plates of mica, sometimes by a film of oil, and sometimes by other means; but still the motion went on. That they were not due to ordinary currents existing in the fluid was easily proved by the great difference in character of the motion resulting when currents were purposely occasioned. That electricity excited by friction or otherwise in known ways was not the cause, was concluded from the circumstance that this motion continued unabated for hours together, whereas there is every reason to believe, according to *what we know*, that a difference in electric state would soon have been equalized, and the particles have come to rest. Other explanations had in turn been shewn, by varying the mode of experimenting, to be insufficient; and naturally averse as Mr. Brown was to leaving the subject in this state, he, after many months' investigation, published the discovery of these motions without pretending to be able to account for the cause. Not asserting that a new power of matter was concerned; not denying that the powers with which we are acquainted might not be sufficient to originate the motion; but thinking it much more philosophical to acknowledge ignorance as to the mode of action in these cases, and to suspend the judgment, than, by the assumption of an opinion, which must have been hypothetical, run the great risk of shackling the mind by the admission of error for truth.

Mr. Faraday then referred to the influence of this discovery of Mr. Brown (which all must acknowledge as true in fact, whatever their theories may be) upon the hypotheses which have been at various times promulgated respecting different points in vegetable physiology, as the impregnation of the ovulum, &c. The discovery of active molecules obtainable from matter which could not be vital, as glass, flint, &c. &c. overthrows all reasoning founded upon the vitality, or even presence of particles which have been supposed to be distinguished by this character. The supposed test is no test, because the character, in place of being peculiar to the particles of organic matter, or, even as some have thought, to the particles from a peculiar organ, belongs to all solid matter; and here Mr. Faraday took occasion to state, in correction of public misapprehension, that Mr. Brown by no means intended to say, or ever had said, that the motion was an indication of vitality, and that, consequently, all matter had life, or that the particles he had made his observations upon were the ultimate particles of matter.

Friday, February 20.

Specimens of ancient armour from Mr. Balmanno were exhibited in the library.

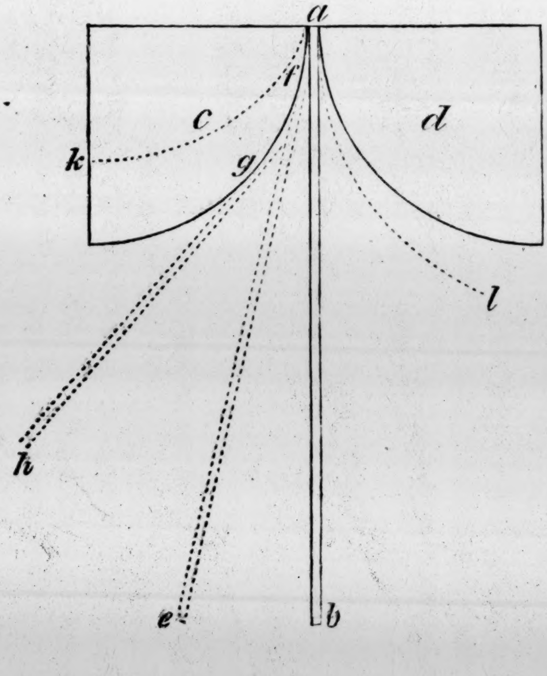
Among the presents were specimens of chalcedony in basalt from the Giants' Causeway, by the Rev. Mr. Caton.

The apparatus for certain phonic experiments were also placed on the table.

In the lecture-room Mr. Ainger explained some of the properties of simple pendulums, and the principle of those contrivances which have been invented for compensating the effect of temperature on the pendulums and balances of time-keepers. With respect to what is called the isochronism of the vibrations of the same or similar pendulums, and which is frequently supposed to be a peculiar property of such bodies, especially fitting them for the purpose of time-keepers, it was shewn that this property is not at all peculiar to the pendulum, for that the isochronism of the pendulum was merely one instance of a very general principle, namely, that perfectly equal and similar actions must of necessity be performed in equal times; which is as true of any other repeated action, as of the vibrations of the pendulum. Any other alternating or repeated action or motion would therefore equally well serve the purpose of measuring time if we could ensure the perfect similarity of such repetitions. The peculiar advantage of the pendulum, therefore, is not in its principle, but in the ease with which it can be freed from those disturbing causes which would prevent the perfect similarity of the successive actions of a more complex body or motion. The simplicity of the pendulum's form, the small quantity of its friction, and the necessary identity of its successive oscillations in every thing but the extent of their arcs, render it peculiarly fitted formeasuring time; the means by which the length of those arcs are made constant by means of the escapement, were explained in a former lecure, the substance of which is given at p. 418, vol. iii. of this Journal.

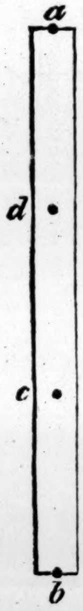
Mr. Ainger then described the means by which Huygens had proposed to render the vibrations of the pendulum isochronous, whatever were the length of its vibratory arc, a mode which, without entering into those mathematical investigations which led Huygens to the discovery, may be thus rendered intelligible by considerations merely mechanical. In the first place, it is known that a short pendulum vibrates more quickly than a long one. In the second, it is also known that the same pendulum occupies more time to vibrate in a large arc than in a small one. Shortness of pendulum and shortness of arc both conduce to rapidity of vibration; length of either produces the reverse effect, or slowness of vibration. It follows, then, that a long pendulum vibrating in a short arc might be found to be isochronous with a shorter pendulum vibrating in a longer arc. It follows, indeed, that within certain limits a diminution of the pendulum's length may compensate for the increase of its vibratory arc, and vice versa. It is not probable that this was the sort of reasoning which led Huygens to his beautiful and scientific invention; but it will serve probably to make that invention more intelligible to most readers than it is generally found. What are commonly called the cycloidal cheeks of Huygens act upon the principle above described; if by any means the arc of vibration be increased, the pendulum becomes shortened during such excessive vibration, exactly so much as will accelerate its motion by a quantity equal to the retardation produced by the increase of its arc.

The figure in the margin may serve to explain this; suppose *a b* to represent the upper end of a pendulum, consisting of a thin, perfectly flexible line, closely embraced at the point *a* by two metal cheeks *c* and *d*. When the pendulum first departs from the perpendicular situation *a b*, it moves from the extreme point *a*; but as it inclines to the right or left, it comes in contact with the metal cheek *d* or *c*, and its motion takes place only from the point where it ceases to be in contact with those cheeks:



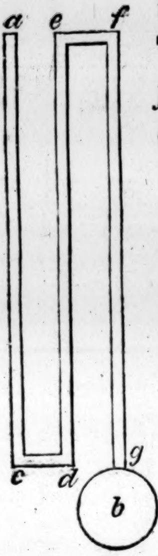
thus, when the position is *a e*, it touches the curved edge of the cheek *c* from *a* to *f*, and thus in this position its virtual length is only *f e*; if the pendulum swing to *h*, it will be in contact with the cheek as far as *g*, and therefore its virtual length will now be diminished to *g h*; for it is clear that the part *a g* is absolutely passive, and might in this position be considered as part of the solid block or cheek *c*. It would be evident, therefore, that a pendulum placed under these circumstances will become a shorter pendulum, as it extends the arc of its vibration; the rate at which it will shorten as compared with the increase of its arc, will of course depend on the nature of the curve given to the cheeks *c* and *d*; for if the curve of *c* were altered to that shewn by the dotted line *a k*, it is clear that the pendulum's length would diminish less rapidly as compared with the increased arc; if the curve of *d* were altered, as shewn by the dotted line *a l*, it is equally clear that the pendulum's length would diminish more rapidly as it increased its arc. Since, then, it is evident that the length of the pendulum may be made to diminish at any rate we please by altering the curve of the cheeks, it is equally evident that some curve may be found which shall make that diminution of length an exact compensation for the increase of the vibration arc. The particular curve which effects this purpose is that called the cycloid; and it is a curious fact that a pendulum, whose vibrations are thus modified, itself describes a cycloid exactly similar to those cheeks which modify it. Such are the general principles of the most elegant and refined contrivance ever introduced into horology, a contrivance whose merit is not in the least affected by the circumstance that it is too refined for practice. The difficulty of making and applying the cycloidal cheeks with perfect accuracy has prevented their actual use; and therefore the attention of clockmakers has been directed rather to making the pendulum describe equal arcs, than to causing unequal arcs to be performed in equal times. This has been effected by means of the escapement described in the lecture before alluded to. One of the chief and most obvious difficulties in the application of the cycloidal cheeks may be worth explaining. In the diagram, the part *a b* of the pendulum is said to be perfectly flexible, and this condition is essential; but perfect flexibility is not attainable even with a riband, nor with any animal or vegetable fibre; there is always a certain portion of elasticity, however small; it is clear, however, that animal or vegetable fibres could not possibly be used as suspenders to a pendulum; metal only can be admitted, and in this material the elasticity of *a b* would be considerable; and the state of tension in which the part *a b* would be placed as it bent against the cycloidal cheeks, would operate like an addition to the force of gravity, and would totally destroy the compensatory qualities of the curve. If indeed *a b* could consist of a metal chain perfectly flexible at every point (which is, of course, impossible) the application might possibly succeed better than it has hitherto done.

M. Ainger next described the nature of the point in a pendulum which is called its centre of oscillation, and which is that point in which, if all the matter of the pendulum were collected, it would vibrate in the same time as before. This will be best explained by an example: suppose *a b* to represent a uniform rod or bar of any uniform substance, suspended from the upper end *a*; suppose it be divided into three equal parts, by the points *c* and *d*. Such a bar vibrating round the point *a*, will perform those vibrations in precisely the same time as if all the material of the bar were concentrated in the point *c*, when, of course, the suspending portion *a c* would be without any weight, and the part *c b* annihilated; so if the bar were roused and made to vibrate round *b*, it would vibrate as quickly as if all its substance were collected in *d*. For these reasons *c* is called the centre of oscillation of such a bar when vibrating round *a*, and *d* would be its centre of oscillation when vibrating round *b*. Every pendulum has its centre of oscillation, but with such complicated forms and various materials as are found in actual pendulums it would be impossible to define its exact place, as is here done for a simple homogeneous pendulum. The most curious and useful fact, in regard to the centre of oscillation, is, however, this: that if the bar *a b*, which was vibrating round *a*, be raised and suspended from its centre of oscillation *c*, it will vibrate with exactly the same rapidity as before. It follows of course, that the bar will vibrate round *d* in the same time as *b*. If then a pendulum vibrating round any point, as *a*, be reversed, and if, while in the reverse position, it is found to vibrate in exactly the same time as before round two other points *b* and *c*; one of those points *c*, will be the previous centre of oscillation, and the other point *b* will be merely the duplicate point to *a*, at the opposite extremity of the bar. The two points *b* and *c* may therefore be easily distinguished from each other, whatever be the form of the pendulum, so as not to mistake *b* for the centre of oscillation to *a*. This property of the pendulum, the reciprocity, or convertibility of the centres of suspension and

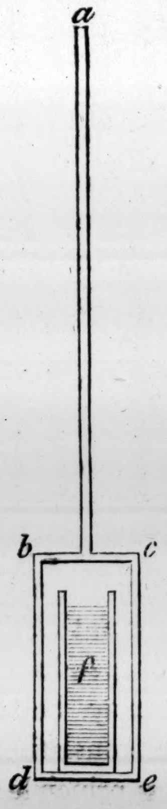


oscillation, had never been applied to any useful practical purpose, till Captain Kater began his investigations into the actual length of the pendulum vibrating seconds, which has of late years become an important problem, in consequence of the use of the pendulum to determine the figure of the earth, and of its being made the basis of the British system of weights and measures. The length of the pendulum vibrating seconds had long been an object of research in France, but the French philosophers had attempted to find it by making a pendulum so simple, that its centre of oscillation could be calculated, and thus measured from the point of suspension; for it should have been premised, that what is called the length of the pendulum, is the distance between the centres of oscillation and suspension; thus, in reference to our bar *a b*, the length of that bar considered as a pendulum is *a c*. But the most simple pendulum may be of different densities at different parts, and even a bar such as is described in the figure may vary sufficiently in its size to affect so delicate an operation as ascertaining the pendulum's length, though no difference should be sensible to the best means of measurement in our possession. It being impossible, therefore, accurately to calculate the centre of oscillation in the most simple pendulum that can be constructed, Captain Kater abandoned the plan altogether, and availed himself of that convertibility of the centres which has been described, and which allowed him to make use of a pendulum as complex as he pleased. This pendulum consisted of a bar having two sliding points of support and other adjustments, which were regulated experimentally till he found two points, from both of which the pendulum vibrated seconds, taking care, of course, that the two points were related to each other as *a* and *c*, and not as *a* and *b*. Captain Kater had then only to measure the distance between the two points, and the distance was the pendulum's length.

Mr. Ainger next described some of the contrivances which had been employed for correcting the effect of temperature upon the pendulums of time-keepers. The elongation of the pendulum in warm weather of course diminishes the rate of the time-keeper, while the shortening in cold weather increases it. The general principle of these contrivances by the use of unequally expansive metals is very well understood; but there is one important distinction between the two most familiar compensation pendulums, which is not so universally understood; and which may be thus explained. What is called the gridiron pendulum, consisting of alternate bars of brass and iron, acts upon the following principle:—



Let a be the point of the pendulum's suspension, and b the bob, as it is called, or weight, at the lower end. The pendulum-rod consists of three bars, $a c$, $d e$, and $f g$. It will be evident, then, that the lengthening of $a c$ or $f g$ would lower the bob b; it is equally clear that the lengthening of $d e$, while the other bars remained unchanged, would elevate the bob, because it would lift the connecting bar $e f$, and with it $f g$; thence it follows, that if $d e$ elongated upwards, just as much as the two bars $a c$ and $f g$ elongated downwards, an exact compensation would be produced, and the distance $a b$ would be constant at all temperatures. Such an arrangement as is here described, besides not placing the weight directly under the point of suspension, would require that the bar e d should be of a metal which expanded exactly double as much as the metal of the bars $a c$, $f g$. Two such metals are not to be found, and therefore the more complex arrangement of the gridiron pendulum was invented by Harrison, in which the quantities of the opposing metals are exactly reciprocal to their rates of expansion, and in which a double series of bars is applied, so as to keep the weight b exactly under the point of suspension.



The mercurial pendulum, which, viewed cursorily, seems to resemble in principle the gridiron, has one very important difference. The mercurial pendulum consists of an iron rod a, attached to an iron frame which supported a glass jar containing mercury, f. When, therefore, the iron rod and frame elongate downwards, the column of mercury lengthens upwards, as the bar d, e, did in the former case. But with this difference:—Suppose the column of mercury, f, to retain exactly its present rate of expansion by heat, but to be solid instead of fluid, and, therefore, not requiring a glass jar to contain it. In this case it will no longer compensate, though its weight and quantity of expansion be unchanged. It will not compensate, for this reason; that now, not being confined in a glass jar, it will expand laterally, enlarging its diameter, whereas, when it was a fluid confined within glass, which expands comparatively little by heat, the lateral expansion was restrained and diverted vertically, so that the column of fluid mercury in a glass jar elongates nearly three times as much as an equal column of equally expansible solid metal; and thus it is that the apparently small column of mercury compensates for the elongation of the whole of the pendulum rod.

Compensation balances for chronometers are, in most popular works, confounded in principle with compensation pendulums, though they are totally different. The compensation of a pendulum tends to preserve the pendulum perfectly identical in length and momentum; the compensation of a balance tends to make it vary in its diameter and momentum. The difference arises this way:—The force which acts constantly upon a pendulum is the invariable force of gravity; this is imitated in a balance by a spring which is coiled and uncoiled by the momentum of the balance, just as the force of gravity is overcome by the momentum of the pendulum. But the force of the balance spring varies with temperature, which is the same as if the gravity of the pendulum altered. The momentum of the balance requires, therefore, to be always changing, while the momentum of the pendulum requires to be constantly the same.

February 27th.

Mr. Faraday, on "Brard's Test on the Action of Weather on Building Stones."

Our readers will find an account of this test at p. 418, vol. xvii., of our last series, and p. 421, vol. iv., of the present. Before proceeding to a consideration of the test, Mr. Faraday gave an account of the action of weather on building materials, dividing the agency of the former into chemical and mechanical, and considering the two apart. The former, although occasionally doing injury, is, as to the materials, and principally in this country, rather advantageous than otherwise; the latter, in these variable climates, is powerfully destructive, the greatest injury arising from the action of water and frost, which is the especial case met by M. Brard's test. Numerous illustrations of the action of weathering were drawn from the public edifices in London and elsewhere in Britain, and also from those specimens of carved stone work which had been brought from the East, and other distant parts, to this country and climate.

After describing M. Brard's test, which consists in the application of saline bodies in solution, so as closely to resemble the effects of frost and mineral salts; and after referring to the minute investigation of its powers lately made in France, Mr. Faraday said he had had occasion to try it on different occasions, and once soon after M. Brard's first publication of the process, and he had found it fully deserving the character of utility which had been given to it.

In the Library were several specimens of Hindu sculpture, which at the same time that they shewed the state of this branch of the fine arts at its best period, (about the eighth century,) also illustrated, by their subsequent history, the point under consideration in the lecture-room. Some African and Burmese musical instruments were also on the table; and, likewise, presented by the author, Mr. Provis's magnificent work on the Menai Bridge.

March 6th.

In the Library were new illustrations of Chinese arts and manufactures, several presents of books, &c., a fine collection of specimens of the vegetable salefiable bases, prepared by Mr. Morson.

In the lecture-room Mr. Brande gave an account of the various changes that ensue during the mutual action of sulphuric acid and alcohol in the process for the formation of sulphuric ether, and illustrated the subject by experiments, tending to explain Mr. Hennell's inquiries into this subject, and his view of the phenomena as described in his papers upon them communicated to the Royal Society. (The subject of this evening will form a paper in a future number.)

March 13th.

Mr. Brockedon offered to the meeting some remarks upon a mode of rapidly sketching effects in chiaro-scuro, and its application to lithography; he stated that the process was similar to that of engraving in mezzotinto, which he thus described. Upon a smooth surface of a plate of steel, or copper, the artist lays what is termed a ground by means of a cradle, a steel instrument, having a serrated edge, which, by a rocking motion, is made to pass all over the plate, and in every direction: this raises a burr, upon which, if printer's ink be laid, it will deliver an uniformly black impression. Upon this rough or raised surface the design is made; and the intended forms are produced by scraping off the burr to the depth of tint required, or if necessary scraping it smooth, that in such parts as no burr remains upon to hold the ink, the surface of the metal would wipe clean, and such parts would appear white in the impression. In a similar way the new process consists in blacking the surface of paper with any mixture of black and a greasy substance, and then with a knife, or scraper, removing it from such parts as are intended in the design to be white or light. Mr. Jackson, the Royal Academician, appears to have been the inventor of this process. Some clever specimens of landscape sketched in this way by Mr. Harding were shewn by Mr. Brockedon, as well as some drawings of his own, of draped figures. One of these was produced in the short space of twenty minutes, and the other in half an hour. The material which he employed was ivory black, prepared in the usual way as for painting in oil; this was rubbed over a white Bristol board with a brush, and the drawing immediately proceeded with. Sketches and studies made in this manner in oil colour, when finished and dry, if worth preserving, may be safely kept, and are not liable to injury from rubbing.

In the application of this process to lithography, lithographic ink is laid over the surface of the stone in a fluid state, and when dry, it is scraped off where required with a mezzotinto scraper, and the designs executed with great facility. Any degree of tint may be produced; parts may be repaired or altered, minute white or black lines, or dots, which it would be difficult to produce on metal, may, in lithography, be made by scratching out with the point of a knife or needle, or drawing dark lines with a camel's hair pencil and lithographic ink, where such lines are required upon places where the ground has been removed. In this way sketches of great boldness, or drawings of extreme delicacy, may be produced. The process of printing from drawings thus executed is certain in its operation, which is not always the case in lithography, when the design is drawn on the stone; as a delicate tint can only be obtained by a fine point on the lithographic chalk and light execution, which, when finished, is liable to be injured by the operation of printing. On the contrary, by this process, the ink being laid on fluid, firmly and perfectly adheres to the stone; it is known that above 100,000 impressions have been taken from lithographic writing which has been laid on with fluid ink. Beautiful and various grain may be obtained on the surface of the stone equal to the tints from metal. The stone employed in lithography is capable of receiving the smoothest surface; but the grain which appears in the print depends upon the degree of the fineness of the sand which is used in finishing the surface of the stone, which may be made to produce a variety and fineness in the grain of the tints similar to those which are so delicate and beautiful in Mr. Martin's celebrated plates in mezzotinto upon steel. Mr. Brockedon acknowledged the assistance which he had received in his experiments from Mr. Hulmandel and Mr. Harding, and exhibited a head the size of nature, which was printed from a drawing made on stone, with which Mr. Hulmandel had obliged him. He also shewed a lithographic print by Mr. Westall, and some spirited lithographic prints by Charlet, a French artist, which displayed many varieties of this new or improved process.

In the Library were a curious and instructive set of models of the agricultural implements employed by the natives of India; they belong to the Asiatic Society. Many fine specimens of Berlin cast iron and several objects of natural history were presented to the Institution.

March 20th.

Mr. Ainger gave an account of the early history of the steam-engine, and vindicated the claims of our countrymen to this most important invention, in opposition to the statements lately put forth in France by M. Arago:—this account will be found in the present Number.

In the Library, Bramah's bank-note press, various models of sounding-boards for pulpits, constructed by the Rev. John Blackburne, and several products of art, were shewn.

March 27th.

The electro-filtration of Porrett, or endosmose and exosmose of Dutrochet, formed the subject of this evening's discussion. Mr. Burnett examined the phenomena, with reference more especially to the light thus thrown on the motion of the sap in plants; and on the real nature of many infusorial animalcules.

He commenced his observations with some remarks on a circumstance which had been long familiar to persons who trade in wines and spirituous liquors, more especially to those who work illicit stills, and such as carry about their persons into prisons and elsewhere wines and spirits in bladders, or, as is usual on the continent, employ vessels made of skins, to preserve them in—that the liquors, if left long in such vessels, diminish in bulk but increase in strength; and indeed this mean is occasionally employed to render them more potent, for the membranes permit the exit of the water, though they confine the spirit, when the liquors are in contact with them; thus raising the quality at the expense of the quantity—a curious fact, and the more remark-worthy, as, under other circumstances, viz.

when the fluid does not touch the membrane, the bladder is not imperviable to the vapour of alcohol, as is well known to all persons accustomed to put up anatomical preparations, for the vases, &c. when covered with bladder, allow the dissipation both of the water and the spirit. Until lately, the cause of this scarcely could be guessed at, perhaps even now it is not fully known; for often are there questions raised, why a specimen of intestine filled with weak spirit to make it sink in the stronger which surrounds it, always becomes flaccid, expelling its contents.

He next directed attention to an experiment, which was certainly the first step towards the solution of the above phenomena, and regretted that a fact so full of interest should have lain so long neglected, and been rediscovered after a lapse of several years by a process so circuitous. The experiment alluded to was one published by R. Porrett, jun. in Thomson's Annals of Philosophy for July 1816, No. 43, p. 75, and had been made eighteen months before. "This experiment, and several modifications of it, I have," said he, "repeated in the laboratory of this Institution, assisted by Mr. Faraday, and the results are here seen. The apparatus consists of a small glass cylinder, divided longitudinally into two cells by a diaphragm of bladder. One of the cells (says Porrett) having been filled with water, and left for several hours, was found to have retained the water. The bladder, therefore, was not sufficiently porous to allow the water to filtrate through it. The cell filled with water was now positively electrified, with a battery of eighty pairs of $1\frac{1}{4}$ inch plates (double), and a few drops of water were put into the empty cell, so as to cover the bottom of it. This small quantity of water was then negatively electrified. The phenomena which ensued were exceedingly curious and instructive. Independent of the decomposition of a small part of the water, which, of course, took place in the usual manner, the principal part of it obeyed the impulse of the voltaic current *from the positive to the negative wire*, first overcoming the resistance occasioned by the compact texture of the bladder, so as in about half an hour to have brought the water in both cells to the same level, and afterwards overcoming the additional resistance occasioned by the gravitation of the water, by continuing to convey that fluid into the negative cell, until its surface in that cell was upwards of three-quarters of an inch higher than in the positive cell." "It is to be lamented (said the lecturer) that the discoverer of this phenomenon should have merely observed, in addition to what I have quoted, that other porous bodies, besides organic membranes, were equally, if not more, effective, and have contented himself with the speculation that the fluid could be made to rise much higher were the electrification continued longer; and with a question whether this hitherto unnoticed power, viz. electro-filtration, is not, jointly with electro-chemical action, in constant operation in the minute vessels and pores of the animal system.

"By thus tracing chronologically, in order to render more intelligible the experiments which are now in course of performance on this table, let it not (said Mr. Burnett) be for a moment supposed that I wish to detract from that merit which undoubtedly belongs to Dutrochet; this author, after a laborious series of experiments, arrived at the fact on which Porrett had fortuitously stumbled, and has filled a volume, nay, two, with ingenious speculations, though not always correct, on that phenomenon, which the other dismissed with a bare and simple question. Dutrochet, a very astute vegetable physiologist, always performing experiments, and ever ready to seize a fact, rediscovered the phenomena already referred to as published by Porrett, but his researches commenced from a different point, and were pursued much further. He found that the minute globulets of a small parasitic moss when loosened and floating in water, the seminal sacs of slugs, and afterwards many other membranes, not only allowed the passage of a fluid through them, but accumulated it on one side, and on that where the denser fluid was. The most satisfactory experiments (these I have repeated, and here are the results) consist of inclosing in small saccules, formed by tying at both ends a part of the intestines of a chicken, a little gum and water, sugar and water, salt and water, &c. and placing these shut sacs, not half full, in vessels of distilled or rain water; when, after a short time, they become quite full and turgid by the passage of the water from without inwards; but, as Dutrochet hath shewn, there is not a single current; for at the same time that an abundant flux inwards is taking place, there is also a less current outwards, which may easily be shewn by testing the water in the vessels in which the saccules float. Subsequently Dutrochet found that a saccule of intestine tied on the end of a small tube, and placed in a vessel, acted in a manner similar to the loose ones, with this additional circumstance, that (as in the experiment of Porrett) the fluid rose in the tube against the force of gravity, and, as he states, (though it did not occur in our experiments) flowed over the open end of the tube. At first Dutrochet believed the flux inwards to depend upon the density, and afterwards upon the alkalinity of the contained fluids; for if the denser fluids or the alkaline were outside the saccules, they became empty; but further experiments led to the conclusion, that electricity was the immediate agent, and that accumulation at the negative pole, which Porrett called *electro-filtration*, Dutrochet named *endosmose*, while the contrary, or outward, current he termed *exosmose*. Dutrochet also found that other porous bodies, viz. inorganic ones, permitted this passage and accumulation: his apparatus for these latter experiments, I presume, must have been formed on the model of Porrett's, whose experiments he quotes; indeed we found that the divided cylinder was the most convenient apparatus for most of the experiments."

Mr. Burnett argued that Dutrochet's explication of the rise of the sap in plants from the impulsion of endosmose concentrated from all the radicles, aided by the affluxion produced by evaporation from the leaves, was not conclusive; as he had ascertained in the leaf-stalks of celery and other plants, that fluids rise in the vessels nearly if not quite as fast when deprived both of roots and leaves as when possessed of both, and under the same circumstances the descending sap is extruded likewise. This was shewn in the ficus elastica, celery, cacti, vine, &c., in which the motion of the sap was notorious, although the cuttings were deprived of both the causes assigned by Dutrochet for its progress. Mr. Burnett contended that besides electro-filtration, which he was inclined to think went far towards explaining the phenomena of absorption, some other agency gave motion to the sap, and this he endeavoured to shew resided in the vessels themselves. Trees, he observed, which have been felled before any of the buds have begun to swell or open, nevertheless will develope their foliage the succeeding spring, fed by the food already deposited in the wood and supplied to the buds; although here is neither leaf to attract, nor root to impel the nutriment. He gave a variety of instances in which such a contractile force is evident in plants, as in the peduncle of the valisneria and cyclamen, the tendrils and stems of the hop, convolvulus, passiflora, Bryonia, and others; the leaf-joints of mimosa, tamarindus, dionæa, &c.; and argued, from analogy, that it might also exist in the individual tubes, for that some such power is resident in them, the before named, and other experiments decidedly evince.

The concluding observations related to the solution of a physiological problem long contested by philosophers, viz., the true nature of those moving corpuscles found in vegetable infusions, and the origin of which has occasionally been thought to favour the doctrine of spontaneous or equivocal generation. These moving molecules, which are commonly more or less of a globular or ellipsoid form, have been detected by many microscopic inquirers. Bory de St. Vincent called them zoocarpes; and Edwards, having detached some cellules from a vegetable body, saw them, when plunged in water, move spontaneously, and thought himself thence justified in concluding that a part of a plant had assumed animality: that which Edwards thought he had analytically, Sir G. S. Gibbes believed he had synthetically proved; for in a paper, published in the Medical and Physical Journal, he states, that about thirty years ago some experiments, which he details, led him to conclude, that growth in plants consists merely in the juxtaposition and union into one body of myriads of monads, which had previously possessed independent vitality, and that the death of any large animal or plant is not truly a decease, but only the solution of a bond which held together many as an individual being, and by which they are all restored to their original independence. In the same manner Dr. Drummond, of Belfast, discovered similar moving bodies by disintegrating the choroid of the flounder and the haddock, even after these fish had been boiled; and he also considered that the belief of each being endued with animal life was the least improbable opinion. But Dutrochet having observed that the saccules, during the expulsion of their contents, acted on the water in which they floated in the same manner as a rocket rises in the air, thus giving to themselves motion, (too lightly considered voluntary, and a sign of their animality,) hence concluded that many of the supposed animalcules, in truth, are not such, but merely detached cellules, owing their motion to the endosmose and exosmose previously explained. This part of the subject was illustrated by comparing the cellules of plants with the pretended animalcules, to which they have a striking resemblance; and, also, by an experiment, in which small globules of glass (candle grenades), filled with ether and floated in water, were shewn to perform just those movements which the suppositious animalcular bodies do: this was designed to prove that mere motion was no incontestable evidence of vitality, and that before the minute molecules alluded to could be considered alive, other more satisfactory witness must be given.

A very numerous collection of specimens of manufactured and other articles from the South Sea Islands were laid upon the library table. They were lent by the Asiatic Society, and had been brought to this country by Captain Dillon, who had collected them during his inquiries into the history and fate of La Pérouse.

April 3d.

Mr. Faraday delivered and illustrated the subject of the evening, which in fact was a continuation of Mr. Wheatstone's communications on the subject of Phonics, and consisted of further investigations of the resonances or reciprocated vibrations of volumes of air. By referring to p. 175 of vol. iii. of this Journal, a general account of the principle of resonances of volumes of air will be found. This was briefly recalled by a few simple experiments, and then the attention transferred to some particular effects produced by resonance of the air in the vocal and oral cavities.

When an original sound is produced, columns of air will reciprocate to it, not only when these vibrations take place with equal rapidity, but also when they are twice, thrice, &c. as rapid, or whenever these vibrations are any multiple of those of the originally sounding body (see page 180, vol. iii.); and it has lately been ascertained by Mr. Wheatstone, that very important qualities are sometimes given to sound in consequence of an effect dependant upon this principle. This was shewn by the Æolina tongue, which in the first place was shewn by experiment to owe nearly all its effect to the resonance of the volume of air confined in the mouth, and in the next, to have the quality and character of its sound very much altered by its vicinity to a column of air, the length of which could be changed at pleasure, so as to cause different multiple reciprocations to the standard sound of the æolina. Many sounds, which have been supposed to be simple, appear to consist of two or more distinct sounds, all after the first and predominating sound being due to additional sounds produced in this way.

An account of the facts detailed and illustrated in reference to the reciprocated vibrations of the volumes of air in the ears, will be found at page 67, vol. ii. of the new series of this Journal; but some others were added to them, and especially the production of very fine grave harmonics, when, the ears being closed, two of the æolina tongues in proper accordance with each other were made to vibrate at the mouth.

During the evening a very singular illustration was given by Mr. Mannin, of the power of producing two simultaneous sounds from the mouth. It was strikingly shewn in some airs which he whistled first as solos, and then as duets. From the command possessed over this power by Mr. Mannin, so as to produce the simultaneous sounds either quickly or slowly at pleasure, no possible doubt could be entertained of the fact. It appeared that at the time, the mouth is divided into two parts by the tongue, and that each portion of air is thrown into a separate state of vibration by the embouchure formed at the lips.

Several Ceylonese musical instruments from the collection of Lady Raffles were upon the library table—the war-gong which belonged to Bandoolah, the Burmese general. Two fine paintings were exhibited by Mr. Gill. Holland's improved microscope, and other instruments, were also shewn.

April 10*th*.

Mr. Ainger concluded his account of the steam-engine.

In the Library were various works of art, Chinese productions in metal, new books, prints, &c. &c.

May 1*st*.

The meeting being resumed this evening after the Easter vacation, Mr. Smart entered on an inquiry concerning the audible qualities of speech, with a view of throwing some light on their original pronunciation of the classical languages, particularly the long lost accent. In all languages there are three things which affect the ear simultaneously—the sounds of the letters; the tone or accent; and the rhythm. Rhythm, which is the same in speech that time is in music, he exemplified by the opening lines of the "Paradise Lost," "Jerusalem Delivered," "Henriade," "Æneid," and "Iliad." The comparative differences are these:—in English and all other modern languages, except French, the rhythm arises from the fixed or regulated accents, independently of any fixed or regulated quantities. In French, there are neither fixed quantities nor fixed accents, and the rhythm arises from the general drift of the sounds; it determines in some degree the otherwise floating accents and quantities, but it is not determined by them. In the present mode of reading Greek and Latin, the rhythm arises from the accents, as in pronouncing English; the place of the accent being determined in both languages according to the rules proper to the Latin language. Mr. Smart then shewed that this was not the original rhythm of those languages: their rhythm arose out of the metrical feet formed of the quantities independently of the accents, the *arsis*, or rising of the hand, making the beginning of each foot, and the *thesis*, or posing of the hand, its completion. To obtain this rhythm by pronouncing words with their true quantities, and yet to give them their proper accents, is the great problem which practice has yet to solve. The lecturer then pointed out the means by which the practice might be recovered by any one who is young and persevering, and endeavoured, by his own reading of hexameter lines, to satisfy his audience of the practicability of his theory.

Mr. Smart then referred to the varying nature of our English accent, when employed to unite words into sentences, and hence drew reasons for many of the mistakes on this subject, and the difficulty of making a fair comparison between accent as now used, and the recorded characteristics of Greek and Roman accent.

In the Library, presents of books from the Royal Society of Literature, and the Royal Asiatic Society, and numerous models of baggage boats, proas, &c. &c., from the East, were upon the table.

May 8th.

M. Faraday described Brunel's block machinery at Portsmouth. There is a set of magnificent models in the possession of the Navy Board, of this admirable invention of Mr. Brunel. They consist of eight separate machines, which work in succession, so as to begin and finish off a two-sheaved block four inches in length. These, upon application from the Royal Institution, were lent by the Navy Board, and were put by Messrs. Maudsley and Field's men (who made them) into such communication and action as to perform the set of operations in the most perfect manner.

Mr. Faraday first gave the general history of the origin of this invention, and of the circumstances connected with the warfare of England with France, &c. &c., under which it was completed. He then briefly explained the particular principle and use of each machine, which at the same time was set to work to perform the operation upon different blocks of wood. He stated generally that the block machinery of Portsmouth, by adjustments, could manufacture blocks of one hundred different sizes—could with thirty men make a hundred per hour; and from the time of its completion in 1804-5, to the present day, had required no repairs from Maudsley, the original manufacturer. The total cost was given at

£46,000, and the saving per annum in time of war £25,000, after allowing interest for capital, and paying the expense of all repairs.

Fine specimens of crystallized carbonate of soda, and sulphate of copper, were laid upon the Library table, presented by Mr. Cookson of Newcastle. A very carefully constructed and correct map of Nubia was presented by one of the authors, H. Parke, Esq. Mr. Soane's designs for completing the public buildings in Westminster were presented by the author. A copy of the death warrant of King Charles was presented by Mr. Netherclift. Messrs. Wood and Moffat's geometrical landscape of public buildings in London was presented by the authors.

Besides these were a great variety of highly interesting curiosities from Persia and the East, lent by Captain Waite, who collected them in those countries.

May 15*th.*

Mr. Mason gave a very clear and plain account of the principles and practice of wood engraving. The object of this evening was to complete the series of illustrations which had at various times been brought forward at the Friday evening meetings, of the different processes resorted to for rendering permanent and multiplying works of art. A highly interesting and numerous collection of specimens was produced in illustration of the subject of the evening.

Spanish ores of lead, presented by J. Mac Donnell, Esq., were upon the Library table. A beautiful picture of Bacchus and Ariadne, in body water-colours, by Mr. Robertson, painted according to a new method invented by himself; and an extensive set of the vegeto-alkalis, and other vegetable principles, by Mr. Morson, were also upon the table.

May 22*nd.*

Mr. Faraday, for Mr. Wheatstone, "on the Nodal Figures of Vibrating Surfaces." The main object of the evening was to describe and illustrate those lines of rest existing in vibrating surfaces, which have been called nodal lines, and which, discovered by Chladni, are rendered evident by the accumulation of small heavy particles, sprinkled over the surfaces before, or whilst in a vibratory state. A nodal point was illustrated by reference to the original experiment and discovery made by Messrs. Noble and Pigott, in the year 1673, when under the tuition of Dr. Wallis, and in which it was shewn that a stretched string could be made to vibrate not only as a whole, but in aliquot parts, the portions being separated by points of rest. A long piece of cane shaken in the air shewed, on a large scale, one, two, or three points of rest, according to the mode of vibrating it, and other experiments for the purpose of making this simple and elementary idea of a node were exhibited.

With regard to surfaces, it was stated that Galileo has first put upon record the knowledge that an elastic surface giving sound has some parts in motion, others at rest; and he directs that, for the purpose of making these parts visibly distinct, pieces of bristle should be strewed over them: he practised this upon the sounding-board of an instrument.

But it was Chladni who, in 1785, observed the beautiful forms into which particles of sand, or metal filings, were arranged by the vibratory motion of an elastic plate. Let a square plate of crown glass, about three inches on the side, be held horizontally by the finger and thumb applied to the opposite central points of the surfaces, and then let a violin bow be drawn perpendicularly over the edge, so as to produce a regular musical sound; if, at the same time, a little coarse dry sand be sprinkled upon the glass, it will immediately assume a regular form, probably a cross consisting of two lines, either parallel to the sides, or to the diagonal, according as the bow has been applied near to the corner, or near the middle of a side. By holding the plate in different places, and in different manners, and varying also the application of the bow, so as to produce different sounds, the forms will be seen to differ, and sometimes as many as twenty or thirty be obtained. These forms indicate the parts where the plates are at rest; the other parts are, of course, in such a vibratory state as to produce sound. They have been carefully traced and recorded by Chladni; and diagrams of complete series of them were placed up in the lecture-room. The general rules for producing the forms, whether upon square, round, triangular, or other plates were also given, so as to place the phenomena quite at command.

Other modes of exciting the plates due to Le Blanc and Savart, and competent to produce new figures, were also described and illustrated.

An account was then given of the production of figures, not upon surfaces directly thrown into a vibratory state, but such as were made to vibrate by reciprocation. These are due to Savart, and by making use of thin extended membranes, he has shewn various new facts in addition to those of Chladni, and especially the possible transformation of figures.

The application of these kind of researches to the investigation of the elasticity of bodies was also referred to, but in a brief manner only; and these, with some new researches upon these points by Mr. Wheatstone, referred to a future evening.

In the Library, amongst the presents, were a very fine specimen of crystallized glass, from W. Cookson, Esq.; and several minerals from Vincent Thompson, Esq. Illuminated Sanscrit and other manuscripts were also on the table, with Burmese books and productions.

May 29th.

Mr. Singer, aided by the very curious collections of Mr. Halsewell, gave a summary view of what is known in regard to those interesting objects of art—THE FICTILE VASES OF THE ANCIENTS. It is well known that these vases have been preserved to our times, some of them for a period of nearly 3000 years, from the circumstance of the prevalent custom of depositing them with the dead in ancient tombs. Two models of these tombs were exhibited by Mr. Singer, shewing the mode in which they were disposed around the corpse, and specimens of almost every variety of the vases so deposited were upon the table. Mr. Singer gave a brief outline of what is known upon the subject; adverting to the forms, the colour, and the decorations of the various vases, commonly called *Etruscan*, but which would more properly be termed *Greek*, as they have principally been found in Greece, in Sicily, and those parts of Italy colonised by Greeks. The subject was further illustrated by an extensive series of the decorations of these vases from the Hamiltonian collection, arranged on a large screen, with diagrams of the principal forms of the vases, and of the seed vessels of certain plants of the Lotus tribe, which Mr. Christie thinks may have been the prototypes of those forms. Mr. Millingen's classification of the vases, according to the subjects represented upon them, was mentioned by Mr. Singer with approbation; and Mr. Christie's suggestion, of the paintings upon them having been derived from the *Sciagraphia*, or transparencies, exhibited in the Eleusinian shows, had also his concurrence.

One very interesting cylindrical vase, the fragments of which were brought from Athens by Mr. Graham, and which was afterwards in the possession of the late Dr. Clarke, of Cambridge, attracted particular attention from the elegance of the design, and from the circumstance of Mr. Wilkin's having made it subservient to the explanation of the Sculptures in the Tympanum of the Parthenon. The results of the analysis of the clay and of the varnish of these vases, by Vauquelin and others, was mentioned. Of the peculiar black varnish no satisfactory analysis has, however, yet been made. Vauquelin thought it might be Plumbagine, or Anthracite; but Pliny seems to advert to a bituminous compound. Mr. Singer said he had reason to think that it was Mr. Faraday's intention to give the subject some consideration, and that the results of his examination might probably, at some future day, be laid before the members at one of their weekly meetings.

A collection of geological and mineralogical specimens from India, was presented by Captain Blake. Ancient stone pottery, dug up from the excavations at St. Catharine's Docks was exhibited; and a copy of an historical flag, representing the emigration of the cinnamon peelers of Ceylon. It was brought from thence by Sir Alexander Johnston, and laid on the table by the Asiatic Society.

June 5th.

Account of an Ascent of the Mont Blanc. By Edmund John Clark, M.D., F.L.S. &c.

The objects of this evening's lecture were to present a clear and faithful sketch, both of the difficulties and the pleasures that occur in this formidable excursion, and then to suggest some admonitions that may increase the enjoyment of future travellers, and lessen the danger both to themselves and the guides.

Dr. Clark described extemporaneously the different aspects presented by this king of European mountains from several magnificent points of view; drew a sketch of the first grand burst of the Alps from the hill of Neuchatel; then of the delicious scenes from Malagny, from the Lake of Geneva and the Hotel de Bellevue at Sallenche; advanced along the valley of Chamouny from its wild entrance above the Pont Pelissier to its opposite end at the foot of the Col-de-Balme, enlivening the narrative by a collection of views; and then explained the formation of the vast Glaciers of the Mont Blanc, and the position of its Aiguilles upon a large model kindly lent for the evening by the Geological Society.

Having arrived at the Hotel de l'Union, at the base of the mountain, the attention was directed to several excursions proper to accustom the traveller to the rarefied air and slippery passages on the ice, that must be expected in the greater ascent. The audience were led along the upper valley of the Mer-de-Glace, by the side of the fearful Moulins, round the base of the Aiguille du Talèfre to the Couverde, and thence to the Jardin, one of the loftiest patches of vegetation in Europe upon an islet of rock in the midst of a wilderness of snow and ice. Several plants of the Jardin, the Couverde, and the edge of the Mer-de-Glace, were placed upon the table*.

The next ascent to the summit of the Buet afforded the description of a scene perhaps unparalleled even in the Alps, whether the eye ranges up the valley of the Rhone towards the Furka and Grimsel mountains, or over the Swiss Alps to the Jura, or across the brown crags of the Aiguilles Rouges to the hoary monarch of the Alps himself, heaving his white head on high to a giddy elevation, opposing his silvery mantle of glittering snow to the deep dark-blue of heaven, and barring in the prospect towards Italy by a dependent chain of inaccessible granite peaks.

Two other excursions to the summits of the Col de Balme and the Breven afforded other views of the mountain, which were at once vividly conveyed by the assistance of large oil paintings †.

After long delay and attentive examination of the more dangerous passes of the Glacier by the telescope, the state of the mountain and of the weather is at last pronounced favourable for the undertaking, the provisions hastily prepared, and the guides all ready to set out. Suggestions as to more suitable provisions, so as to lessen the labour of the guides, by carrying articles less bulky, but more nutrient, such as pemmican, portable soup, essence of coffee, prepared cocoa, sago, rice, &c., claret in some vehicle lighter and less fragile than glass, alcohol in case of emergency, and alcohol-lamps to melt snow in a light metallic vessel, &c. Pleasant route on mules by the side of the Arve to the Hameau des Peberins; examine the barometer there, and appoint the hours for repeating the observations; commence the ascent by a winding path through a pine forest, passing Favret's cottage; leave the mules at the Pierre Pointue; stop to breakfast at the Pierre de l'Echelle. Collection of plants from the base of the Mont Blanc to the base of the Aiguille du Midi, laid on the table.

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* Achillea nana.
Senecio incanus.
Saxifraga Bryoides.
——— aspera.
——— oppositifolia.
Sibbaldia procumbens.
Tussilago alpina.
Ranunculus glacialis.
Empetrum nigrum.
Azalea procumbens.
Alchemilla pentaphylla,
——— vulgaris.
Avena Versicolor.
Ranunculus Pyrenæus.
Juncus Trifidus.
&c. &c.

† These were executed expressly for the occasion, with equal spirit and fidelity, by M. de Chastelain, an artist of much promise.

After resting at breakfast pulses counted, and still found to be accelerated, varying in the different subjects from 84 to 108.

Prepare for leaving the solid rocks; put on the ice-screws; cross the Moraive, and enter the Glacier; minute details of its difficulties, dangers, and beauties; mode of crossing the ice-chasms; advantages of using the ropes early; those now employed too thick and heavy; suggestion of lighter and tougher materials; objections to the veil; wire-gauze masks, with blue spectacles, better; proposals of de Saussure's guide to take a parasol and bottle of essence not ill founded; painful effect of the sun's rays, and its causes; march of more than four hours across the ice; very difficult chasms at the base of the Grand Mulets; unpleasant and hazardous mode of climbing to the top of these rocks; better to avoid it by making a wide circuit.

	Pouce	Lines.	
Barometer on the Grand Mulets .	19	8	French.
Thermometer of barometer . .	50°		Fahr.
Exterior air . .	51°		Fahr.

Encampment for the night; hints to render this night bivouac more luxurious; singular and sublime effects of sunset; large avalanche; moonlight scenery; altered appearance of the sky and stars.

Early on the second morning leave the Grand Mulets for the summit. Plants and mineralogical specimens of these rocks laid on table*.

Snow hard and firm; pass by several immense abysses in the Glacier; stopped by a very wide deep chasm; coast along it for some time, and at last cross over it with much trouble and danger on a bridge of poles; fatiguing ascent in deep snow to the Grand Plateau; headache, disposition to sleep, and nausea commence; debris of an immense avalanche; approach the spot at which the three guides perished in 1820; minute details of that fatal event from the lips of the guide Julien, who, with our captain, Marie

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* The Phyteuma Hæmesphericum was found in blossom, and on the mineralogical specimens brought down by Dr. Clark from the Grand Mulets, several lichens are found:—Cornicularia Pubescens—Acharius; Lichen Pubescens—Lightfoot; Parmelia Cycloselis; Gyrophora Cylindrica—Acharius; Lichen Crinetus—Lightfoot; Lichen Geographicus, &c.

Coutet, fell into a chasm close to that in which the three brave men were killed; new route traversed by Mr. Fellowes in 1827, on the east of the Rocher Rouge, by which the danger of avalanche is avoided*; Mr. Auldjo's opinion that the summit of the Mont Blanc will be inaccessible for some years, considered; when Mr. Auldjo ascended he found the Grand Plateau barred by impassable chasms, and pursued the new route of Mr. Fellowes to the summit; thoughts on the supposed augmentation of the glaciers, and on the prevalent opinion of the guides as to their septennial increase and diminution.

Slow and laborious ascent of the steep icy cone of the Mont Blanc; overpowering tendency to sleep; excessive labour in cutting steps in the ice; shorter passage directly up to the Derniers Rochers; sudden burst of the wide landscape of Italy; overwhelming fatigue, great difficulty of respiration, violent headache, and exhaustion in climbing the last slope; arrival at the summit, the utmost pinnacle of Europe; account of sensations and phenomena observed on the summit; various effects of the rarefaction of the air; sounds feebly conveyed; altered appearance of the heavens; the Ansonia butterfly occasionally found to fly over the very summit of the Mont Blanc; Mr. Fellowes observed a butterfly pass rapidly over his head, and conceived it to be the Bombyx, or Pavonia Major. We observed two large birds, the Corvus Graculus stoutly flying over the Rocher Rouger; Mr. Clessold observed a dead bee on the snow beneath the Rocher Rouge†.

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* In 1826, Dr. Clark suggested and published the possibility of ascending in this direction; these are his words: "I should think it well worth investigation, whether there was not to be found a passage east of the Rocher Rouge. If it be so, then both the labour and the danger will be diminished." Mr. Fellowes's discovery was, however, independent of this remark, and is of much importance, as it removes what was once the most dreaded of all the dangers.

† *Friday Aug.* 26, 1825.—*Barometer on the Summit.*

Pouces Lines. English Inches.
15 9.6 = 16.8393240.
Therm. of Mercury — .5 Reaum. = 30.875 Fahr.
Therm. of Air — .75 Reaum. = 30.3125 Fahr.

Barometer, Lower Station.—Valley of Chamouny.

Pouces Lines. English Inches.
25 1.1 = 26.7421965.
Therm. of Mercury, 14° Reaum. = 63.5 Fahr.
Therm. of Air, 14 Reaum. = 63.5 Fahr,

Much

Panoramic description of the immense and awfully sublime prospect from the top of the Mount Blanc, looking over the Swiss, Savoy, and Tyrolese Alps; over the wild plains of Lombardy to the Apennines; over the silver meanderings of the Po towards the Superga and Genoa; across the Tarentaise; beyond Chamberi and Lyons; over the lakes of Geneva, Morat, Neuchatel, and Bienne, to the line of the Jura, with here and there a glimpse of the plains of France beyond; more detailed views of the colossal Aiguilles of the Mount Blanc itself now all lying directly under the eye. Observations suggested by this view. Specimens of all the rocks occurring in the line of ascent, laid on the table in the order in which they occur, with a specimen of the loftiest rock on the Italian side, composed of felspar, hornblende, chlorite talc, and a little quartz. See Brochant, Annales des Mines, 1819, on the Granitoid rocks of the Mount Blanc, in which the hornblende is melted into numerous vitreous bulbs. Dr. Clark placed on the table a specimen covered with these bulks, believed by de Saussure to be the effect of lightning; a section of the same specimen shewing, that this effect is altogether external; and a small portion of the same rock formed artificially in a similar manner by the blowpipe.

Various mineralogical specimens from the southern side of the Mont Blanc, chiefly found on the Glacier de Nuage and its vicinity, were also arranged on the table; but although at the request of a crowded audience the lecture was continued beyond the usual hour, it was found impossible to complete the subject; and it is probable that the paper will be resumed at an early period in the next session of the Institution, and may afterwards form the nucleus of a separate publication.

The library tables were furnished as usual with various novelties in literature, science, and art.

Much praise is due to Captain Sherwell, who, though quite exhausted with fatigue and nausea, exerted himself to complete this barometrical observation. His uniform gentleness and kindness, both during the ascent and subsequently, in communicating specimens and other memorials, impressed Dr. Clark with a very strong feeling of obligation. The mean of the results of these data wrought according to the formula of Dr. Robison, M. de Luc, Sir George Shuckburgh, Dr. Hutton, Professor Littrow. See 'Trans. Astron. Soc.,' Vcl. i. p. 299; and by the accurate Tables of Mr. Francis Baily agrees very nearly with the trigonometrical results stated in the Correspondance du Baron de Zach, tom. x. p. 522.

June 12th.

At this meeting the subject of the manufacture of glass for optical purposes was discoursed upon by Mr. Faraday.

It is pretty generally known that a committee and a sub-committee of the Fellows of the Royal Society have been formed to experiment upon the production of glass fit for optical purposes, *i. e.* free from blebs, spots, streaks, and, above all, waves and striæ, so that it may have a uniform action upon light. The sub-committee consisted of Messrs. Herschel, Dollond, and Faraday, who each took those respective parts of the investigation which accorded with their particular pursuits. The Royal Society some time since applied to the Royal Institution for leave to erect upon the premises of the latter a furnace and furnace house for the performance of numerous experiments of investigation, which was immediately granted, and which has now been in operation twenty-one months. Mr. Faraday stated that although he ought to make the first official report of his progress to the president and council of the Royal Society, yet he had been permitted, in consideration of the present evening being the last of the meetings of the Royal Institution, to anticipate, and lay before the members of the latter, in a verbal report, an account of his proceedings.

A brief account was then given of the great discovery made by John Dollond, in 1758, of the construction of achromatic object glasses; of the consequent demand for good glass; of the attempts of Guinand, Frauenhofer, Bontemps, Lerebours, &c.; and of the difficulties, great as ever in this country, if not also in France, of getting good glass.

Mr. Faraday then minutely described the nature of the peculiar optical glass which, for the last nine months, has had his undivided attention, and also the process of working it, which was illustrated by all the apparatus. Ordinary flint glass may be considered as a compound of oxide of lead, silica, and alkali. Faraday's heavy glass is a compound of oxide of lead, silica, and boracic acid: alkali is found to be very injurious in it. It is readily fusible at a red heat, and after the materials, perfectly pure, are mixed and fretted into a rough glass in earthenware crucibles, the required portion is transferred into a platina vessel, and completed by stirring and other operations until it is perfectly uniform and clear, and fit to be cooled and annealed.

Mr. Faraday stated that he had been racing against time, and had not lost the match, for that a piece of glass, which he hoped would prove nearly unexceptionable, had been taken out of the furnace, and was in the hands of Mr. Dollond to be manufactured into a telescope. It had indeed been formed into an instrument, and by such inspection as time had allowed, was good, but it wanted a still more rigid examination. Three telescopes of this glass had been made; every instrument was an improvement upon the former, and the experiments generally had advanced towards perfection from first to last. Although very anxious not to excite hopes which would not be justified in the fullest manner, still the doubts Mr. Faraday entertained of perfect ultimate success seemed very slight.

In the Library was a very fine coral (Venus' fan) presented by Mrs. Jones; and also the Canterbury Pilgrims and Procession in Ivanhoe, presented by Mr. Henning.

There were likewise some pieces of oak timber from the New Forest, which had been experimented upon as to strength, accompanied with drawings and notes of the results, laid upon the table by Mr. Hellyer. The following is an account of the experiment:—

In a violent storm in July 1828, the lightning rent out a very long strip, of about two inches wide by one in thickness, from the heart of a fine oak, growing on an elevated spot in the forest. Nearly one quarter of the tree was forced away from the body of it, and several of the massy limbs of the upper part were driven, as it were, from the sockets, a distance of several feet, and either lodged upon the lower limbs, or came at once to the ground.

The heart thus separated attracted particular notice. Its great toughness struck every one; and this circumstance, together with the general estimation for superior strength, in which the oak timber of this forest is held, led to the following experiment.

The object was to ascertain by actual experiment, made upon a large scale, what weight, laterally applied, the oak timber of the New Forest was capable of resisting.

For this purpose, by command of the first Commissioner of his Majesty's Woods and Forests, a seasoned stick of timber was selected from the timber in the wood yard of the Crown. The tree was cut down in April, 1827; and it grew in a quarter of the forest called Stubby Copse. From about midway between the centre and circumference of the tree, and beginning at about four feet from the ground end, a piece of very good and perfectly sound timber was cut, and reduced to the dimensions of five inches square, and eleven feet long.

The place chosen for making the experiment was between two large trees, which were standing at a convenient distance from each other. Here a four-inch plank was partly sunk into the ground, to produce a level even surface. On the plank two uprights, made each of a portion of the trunk of a lesser tree, were erected; and these uprights were braced and firmly supported, so as not only to bring their inner sides to the exact distance of seven feet apart at their superior extremities, at which the bearing lines were formed, but also to maintain them in that position, and at that distance invariably, during the application of the force.

The piece to be tried was laid across the two uprights; and a rough scale-like platform to contain the weight, formed of a very large plank, was suspended from the centre, by a strong timber chain. Upon this platform, piece after piece was laid of hard Purbeck stone, until it became evident that there was sufficient to effect the fracture, and in a few seconds the whole was borne to the ground. The stones employed were then weighed off, at the scales in the bark shed, and the weight of the platform and chain being added, it was found that the aggregate weight by which the object had been obtained, was 9,061 pounds, or 4 tons, 3 quarters, and 17 pounds.

Several other particulars were observed, but to introduce them here would be to extend this notice beyond the present occasion.

The experiment was made in the presence of the First Commissioner of the Woods, and the Secretary of the Board, of the Treasurer of the Ordnance, and of Mr. Hellyer, at whose suggestion it was undertaken; and the preparations for it were made by the Deputy Surveyor of the Forest and his assistants.

Two portions of the piece broken were on the table, shewing the fracture.

Several mechanical models were upon the table—an original painting in body water-colours, by Sir James Thornhill, of the west window in Westminster Abbey—and restorations by M. Thon, of Il Tempio della Fortuna Prenestina, and Il Palazzo dei Cesari.

This was the concluding evening for the season of the Friday meetings of the members of the Royal Institution.

MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *Elasticity, Ductility, and Strength of Hammered and Rolled Iron.*—The following are the principal results obtained by M. Lagerhjelm, with great care, and the use of a very complete and powerful apparatus:—

i. Rolling always gives the same iron the same uniform density. Hammered bars of the same iron are often of different densities, and frequently contain scales. ii. Rolling does not twist the fibre of the bar: hammering sometimes does. iii. The measure of elasticity is the same for both hammered and rolled bars; but the limit of elasticity (measured by the greatest weight which the bar can support for a given sectional surface, without any permanent change of form) is greater for hammered than for rolled bars, if neither have been refolded; but if they have been refolded, the limit of elasticity is increased, and becomes the same for both. iv. Rolling gives more ductility to iron than hammering. v. Cohesion appears independent of the process employed, and is the same for both. vi. The lengthenings and shortenings (both of which follow the same laws) are not proportional to the forces which draw or compress a bar of iron in the direction of its length. vii. Elasticity is not changed by tempering. viii. Very different forces are required to produce the same permanent change of form in a brittle and a soft iron. ix. The limit of ductility being taken as the length which a bar a foot long will increase by, from the state of unaltered elasticity until the moment of rupture, the most ductile iron experimented with was found to give for its limit of ductility 0.27 of the original length, and 0.722 of the original section. x. The cohesion is the same for brittle or soft iron, fibrous or not fibrous: so that the absolute strength of iron appears to depend upon its ductility. xi. The volume of the metal increases as the bar is drawn apart; and the specific gravity of the iron at the broken surface is less nearly by 0.01 than that of the same iron taken from an unaltered part of the bar. xii. When the iron is extended, preparatory to its fracture, heat is evolved: the heat is greater for soft than for harsh iron. Sometimes a bright spark appears at the moment of rupture.—*Bull. Univ.* E. x. 42.

2. *On Magnetism by Rotation.*—A very careful set of experimental investigations by M. de Haldat, of Nancy, on this subject, has been published, in which are set forth the endeavours made to ascertain why rotatory plates should affect magnetic needles suspended near them; and, if because of magnetism induced in them, what is the original source of the power? The conclusion is soon drawn, that the obedience of the needles to the motion of the plate is a consequence of the magnetic state of the latter; but the source of this magnetism was not so evident. In the first place, it was ascertained that

the feeblest magnetic needles were affected by the rotatory plates, and the conclusion established, that every polarised needle having a directive force, obeyed the moving discs, until that force was reduced to zero. It was then ascertained that very feeble magnets readily developed magnetism in other bodies susceptible of receiving and retaining the power; the weakest needles rendered other needles magnetic, and even produced sensible traces of the power in bars a hundred times their weight. As magnetism is thus communicated with extreme facility, there was no reason to doubt but that if the rotatory discs became magnetic merely by rotation, their magnetic power could be rendered evident; but unmagnetised needles placed near to, and even in contact with, the plates when revolving, even with their utmost rapidity, obtained no magnetic power, although the position was altered in every possible way. The disc was equally inefficacious, when moving under the influence of a strong magnet: if, therefore, its power over needles be attributed to magnetism developed in it, its nature must be very peculiar not to allow of communication, or else it must be far weaker in power than the feeblest needle that could be constructed; but then, how is it possible that it should force needles and magnets round, notwithstanding their strong directive force?

Supposing that the want of success in these trials might be due to the coercitive force of the metals used in constructing the discs, *i. e.* the force which the metal opposes to either the reception or the loss of magnetic power; the comparative effects of different metals were examined. Soft iron acted with more energy than copper. Iron well hammered when cold, as well as soft iron, did not possess communicable magnetism; but a disc of untempered steel being used, M. de Haldat was astonished to find, that, instead of being more affected, because of the increase in coercitive force, the needle over it, after a few oscillations, pointed directly to the north; and the more rapid the motion of the disc, the more invariable the position of the needle. Hence it appears that the *drawing force* exerted upon the needle is inversely as the coercitive force of the metal used.

By attention to circumstances, M. Haldat was able experimentally to extend the influence of the disc upon the needles to as much as one inch and a half and more. He also found the best method of shewing the effect was not by continued rotation at first, but by an oscillating motion given in equal times with the natural vibrations of the needle, so as to accompany and urge them. The needle was first made to vibrate in small arcs, and when these had been increased, by the oscillations, to 180°, then the continued rotation was adopted and retained. In this way, the influence of substances over the needle may be ascertained, which cannot be rendered sensible in the ordinary manner. In the same way also, strong oscillations of the needles were obtained with a piece of brass, not circular but prismatic, 5.9 inches long, nearly 2 inches wide, and 0.315 of an inch thick.

Whatever the position of the needles and the plates to each other, or to the earth, their mutual action was the same, so that, whatever may be the direction of the magnetic current to that of the motion of the disc, the phenomenon of rotatory magnetism still appears.

On raising the revolving plates to various temperatures, even up to a red heat, still no difference was observed between their action then and at ordinary temperatures. These experiments are difficult of performance, but M. Haldat intends to pursue them in an unexceptionable manner, especially for the purpose of observing what will happen with a steel plate, which at high temperatures will, in coercitive power, be brought to a level with soft iron, and into the same condition with it.—*Ann. de Chimie*, xxxix. 232.

3. *Refraction of light by bodies in different states.*—One of the consequences deduced by Newton from his theory of light, is that the refractive powers of bodies of the same kind, but of different densities, is proportional to the density. Exceptions to this law were first shewn by MM. Arago and Petit, who found that the vapours of ether, and carburet of sulphur, had refractive powers, which, compared with those belonging to the same substances in the liquid state, were less than they ought to be when calculated from the comparative density of the fluids and their vapours.

More lately, M. de la Rive has examined the same point in the theory of light, by reference to experiments on the condensible gases. Liquid sulphurous acid, for instance, he finds to have a refractive power a little greater than that of water, whereas, calculating from the refractive power of the same substance as a gas, ascertained by M. Dulong, it ought to have, in the liquid state, a power only of 0.661; but water has a power =0.784, and, by experiment, sulphurous acid is still greater, hence another instance in addition to those mentioned above.

Calculating in a similar way, and from the experiments of M. Dulong upon the gases, what ought to be the refractive power of liquid *ammonia* and liquid *sulphuretted hydrogen*, they come out as 0.752 and 0.767, both of which are less than 0.784 or the power of water, although, according to Mr. Faraday's experiments, both these condensed gases have a greater refractive power than water.—*Bib. Univ.* xl. 207.

It may be doubted, from the difficulty attending the only method in which Mr. Faraday could take the specific gravities of ammonia and sulphuretted hydrogen, whether the results are sufficiently accurate for conclusions like those above. The densities are given merely as approximations; and it is evident the bulbs he used must have been altered by the pressures to which they were subjected.—*Ed.*

4. *Horizontal Force of a Magnetic Needle.*—It has been announced to the Academy of Sciences by M. Dulong, that M. Babinet has determined the horizontal magnetic force of the earth by a method derived from that invented by Poisson in 1825, but by experimental processes and calculations different from his. He has not resorted to

the oscillations of a magnetic needle, but obtained the magnetic force by the torsion of metallic wires, and by the isochronous vibrations of copper needles of a given weight. The force of the earth is almost entirely compensated for reciprocal actions, by the direct torsion of a properly selected wire, which is fixed below the lever carrying the moveable needle. Ultimately, M. Babinet arrived at the following theorem:—A magnetic pole, which, at the distance of a metre (39.37079 inches), acts on an equal pole with the force of one milligramme (0.0154 grains), is directed by the earth with a horizontal force 320 times as great (*i. e.* 4.928 grains.)—*Revue Encyc.* xli. 827.

5. *On the flowing of Sand under Pressure.*—A year or two since, M. Huber Burnand described an anemometer to the Philosophical Society of Geneva, in which the force and duration of the wind were ascertained by the quantity of sand which escaped from an aperture varying in its size with the wind. In consequence of conversation relative to the manner in which sand would flow, and how far it would accord with liquids in this respect, M. Huber undertook a set of experiments which have given some very curious and interesting results: these we shall endeavour very briefly to abstract.

To obtain a regular fall of sand, it must be well sifted, but must not be too fine. That which will just pass through a sieve containing 38 wires per inch in one direction, and 45.6 in the other, flows with great facility; but the aperture must never be less than .079 of an inch in diameter. The sand, in the experiments, was generally contained in wooden boxes, closed at the bottom by four moveable plates, so that the aperture could be widened or lengthened at pleasure, the edges of these plates being bevilled on the exterior. The sand which had passed the aperture was estimated both by weight and measure.

The quantity of sand which flows in a given time through a given aperture is the same, whatever be the volume, the weight, or the height, of the sand in the box above. The height was sometimes increased tenfold without change.

When the aperture was from .078 to .118 of an inch in width, the quantity of sand which passed was always directly as the length of the aperture; but the least increase in the width occasioned an increased quantity greater than in the direct ratio of the areas of the apertures. The first part of this result may be useful in the construction of numerous instruments.

Sand passing out by lateral apertures made in the *surface* of the boxes flowed with equal velocity, whatever the height of the column within; but if these lateral apertures were made in the form of horizontal holes, and the diameters were not nearly equal to the length, not a grain of sand would pass.

Sand being poured into one leg of an inverted syphon did not mount in the other. It extended but a very little way into the horizontal part.

Whatever pressure be given to the upper surface of the sand, it exerts no influence on the issue below. From a quarter to half a hundred-weight produced no difference.

A rod placed upright in the sand, directly over the aperture, descended without any inclination, in a very regular and uniform manner. When placed between the centre of movement and the border of the box, it also moved regularly, but gradually became inclined.

M. Huber remarks that there is perhaps no other natural force on the earth which produces by itself a perfectly uniform movement, and which is not altered either by gravitation or friction, or the resistance of the air: for the height has no influence; friction, in place of being an obstacle, is the regulating cause; and the resistance of the air, within the column, must be so feeble as to be altogether insensible as a disturbing force.

Single grains of sand placed upon a flat surface did not roll until the inclination of the surface was at least 30°, sometimes almost 40°. The angle formed by a heap of flowing sand is almost always between 30° and 33°; very rarely as much as 35°. Small shot and pease produced the same general effect as those described with sand.

An egg was put at the bottom of a box, and covered a few inches deep in sand: a weight of 60 lb. or more placed over it did not injure or affect the egg. The same result took place when the sand at the bottom of the box was put in motion by opening the aperture. Hence it is evident that the sides of the vessel support the pressure almost entirely; and also that the egg is protected from the irregular action of the pressure by the sand, to the same degree, though not in the same manner, as if in a liquid.

Other experiments fully proved that the pressure of the sand was principally sustained by the sides of the vessel. An inverted syphon had a little mercury poured in until both limbs were partly filled, and then sand was poured in on the one side upon the mercury: no elevation of the mercury in the other leg took place. Pressure was added to the weight of sand, still the mercury remained unmoved. More delicate experiments, by means of balances, showed the same effect.

A simple paper tube was rolled up; a plug fitting very loosely into the end, but not so that sand could pass it, was floated by a cork or otherwise, on water, with such buoyancy as to be able to sustain the little cone of sand that its circular surface could receive, and then the tube was held in the hand over it. On pouring sand in, the tube might be filled without displacing the floating plug below, although there was weight enough to sink plug and tube, and every thing else, if the hand had been taken away. The sides of the tube, in fact, supported the sand.

From these, and other experiments, it was concluded that it would be exceedingly difficult to *push* sand out of a tube; and upon trial, this was found impossible: the sides of the tube gave way first.

Even when inclined 20° downwards it could not be done. It is easy, therefore, to see how, in the blasting of rocks, sand is as effectual for closing the hole as a plug, the most compact driving, or any thing else.

A very simple mode of shewing the most striking of these effects is to prepare an open tin tube, an inch in diameter, and of any length; to press a piece of thin paper against the end of it with the hand; to moisten the edges of the paper; and, replacing it, to let it adhere simply by such moisture; then placing the tube upright, with that end upon the table, to fill it with sand, and afterwards gradually to raise it. Whatever the weight of the tube and its contents, it may be carried any where in that position, although the paper bottom adheres but very slightly.—*Bib. Univ.* xl. 22.

6. *Improved Method of fixing the Mariner's Compass.*—This improvement is due to Mr. Lemuel Langley, of Norfolk, in the State of Virginia. It is the object of a patent, and is thus described:—"The object of my improvement is to dispense altogether with the binacle in which the compass is ordinarily fixed; to cause it to answer all the purposes of a tell-tale, and to secure it against accidents from cannon shot, the shipping of heavy seas, or any other cause of injury. The mode in which these ends are attained is by cutting a hole through the deck of the vessel, at or near the place where the binacle is usually situated. This hole is cut through into the cabin, and within it is placed the compass, with its box, suspended in the usual way; and when so situated, it is completely out of the reach of cannon or other shot. In order to cause it to act as a tell-tale, the compass box is made with a glass bottom, so that the card can be seen as perfectly in the cabin as upon deck. I also make the compass card translucent, or semi-transparent, in consequence of which it may always be lighted from below, and will be much more plainly seen at night than when lighted in the ordinary way. The compass is defended at the top by a very thick piece of glass: such as I have used has been three-fourths of an inch in thickness; and this is also defended by a rim or band projecting above the deck. The lower side of the box is also glazed; and I contemplate sometimes making the sides of the box of glass, should it be desirable to admit light that way."

The editor of the Franklin Journal speaks strongly in praise of these compasses.

7. *Smoke Disperser of M. Millet.*—A report upon this apparatus, made by M. Derosne, speaks favourably of its powers. The apparatus is simple, consisting of a kind of tub pierced with a great number of holes, having the burs outwards. It has been taken into practice by many persons. In order to prove its efficacy, one of them was fixed on the top of the funnel pipe of a stove, and a very close, smoky fire made below. By means of a ventilator, an artificial wind was then made to strike directly and powerfully on the smoke disperser for the

purpose of driving the current downwards and making the stove smoke; but neither by this, nor even additional means, could any effect of the kind be produced. The committee could not explain the effect, except by supposing that the cylindrical form of the apparatus presenting only a small surface to the action of the wind was favourable, and that the form of the small apertures in this cylinder, occasioning a great number of contrary currents, produced almost a perfect neutralization of the force of the wind. Whatever may be the cause, the apparatus offers a cheap and effectual remedy for smoky chimneys, when this fault in them is dependent upon the pressure exerted by winds upon the upper aperture of the flue.—*Bull. Soc. Enc.*

8. *Enamelled Street Names.*—Recently the names of the streets in Paris have been put up on enamelled plates, the ground being blue and the letters white. The substance on which the enamelling is performed is lava in slabs, and its use was suggested by the Count de Chabrol. The prepared tables have been submitted to Brard's test, and also to great differences of temperature, without suffering injury. The same substance (lava) has since been used as the basis of certain enamelled designs; it is much superior in some points to porcelain in this application, because the necessary exposure to fire does not cause it to crack in the manner that porcelain does, and which often, with the latter substance, is followed by the destruction of the artist's work.—*Jour. de Paris.*

9. *Examination of Patent Claims.*—Since 1826, the applications for patents in Piedmont and Sardinia have been referred to the Academy of Sciences at Turin, with the intention of negativing all those founded in ignorance or knavery, and supporting only those which are true improvements, and in their effects really advantageous to the arts. Although the number of patents has been excessively diminished in consequence, yet it appears that the applications have increased in an extraordinary degree. The academicians, though much engaged in the year 1826 in examining claims, were poorly rewarded by the occurrence of actual improvement: only two machines were brought forward which received their sanction.

10. *Detection of Potatoe Starch in Wheat Flour.*—M. Chevalier says, that when flour, adulterated with potatoe starch, is sprinkled upon black paper, and examined by a powerful lens, or a microscope, the starch may be detected by the brilliancy of its grains.

11. *On Adulteration of Wheat Flour, by* M. Henri.—Many varieties of flour have been submitted to us to ascertain the presence of potatoe starch in them. By the aid of a good lens it was easy to see certain brilliant and crystalline points in them; but not being in this way able to ascertain the proportion mixed with the wheat, we thought it would be better to determine the quantity of gluten in them, and

compare it with the quantity obtained from wheat flour prepared under our own inspection.

Thirty specimens of flour, from the harvests of 1827 and 1828, were, therefore, examined, and the gluten separated; and without taking any note of the other principles present, we found that, upon a mean, they gave 10.25 per cent. of gluten, perfectly dry and pulverized; whilst the flour, supposed to be adulterated, gave only from 6 to 6.5 per cent. of gluten in the same state. Hence it will be easy, by an operation of this kind, to tell whether a flour has been adulterated or not.—*Jour. de Pharmacie.*

12. *Remark on a Passage of the Mècanique Analatique of Lagrange, by* N. G. de Schulten.—Having attentively examined the beautiful theory of the equilibrium of an elastic thread, given by Lagrange, in the first volume of his Mécanique Analytique, p. 151, 159, Ed. Paris, 1811-15, I have found that an error has crept into it, to the correction of which I would the more especially direct the attention of mathematicians, as it exists in a work which is generally and justly regarded as classical, and is latent there under so plausible an appearance, that it may have escaped the penetration even of the celebrated author to whom analysis is so deeply indebted.

The error in question regards the general expression for the tension of an elastic thread, composed of an element of an *invariable* length, which the author finds to be different from that which holds good for a thread with extensible or contractile elements, the difference being, according to him, such, that if in each case the tension be denominated λ, the equations of equilibrium are in the first case—

$$\text{i.}\quad\left\{\begin{array}{l} Xdm - d.\dfrac{\lambda dx}{ds} + d^2.Id^2x = 0 \\ Ydm - d.\dfrac{\lambda dy}{ds} + d^2.Id^2y = 0 \\ Zdm - d.\dfrac{\lambda dz}{ds} + d^2.Id^2z = 0 \end{array}\right.$$

While in those of the second they are—

$$\text{ii.}\quad\left\{\begin{array}{l} Xdm - d.\left[\left(\lambda + d.\dfrac{Ed^2s}{eds^2} - \dfrac{Ee}{ds}\right)\dfrac{dx}{ds}\right] + d^2.Id^2x = 0 \\ Ydm - d.\left[\left(\lambda + d.\dfrac{Ed^2s}{eds^2} - \dfrac{Ee}{ds}\right)\dfrac{dy}{ds}\right] + d^2.Id^2y = 0 \\ Zdm - d.\left[\left(\lambda + d.\dfrac{Ed^2s}{eds^2} - \dfrac{Ee}{ds}\right)\dfrac{dz}{ds}\right] + d^2.Id^2z = 0 \end{array}\right.$$

Now I affirm, that in both one and the other of these two cases, it is only the equations (ii.) which hold good. The (i.), although leading, as may be readily seen, to true equations of the curve of the thread, nevertheless do not give the true value of the tension. The detailed analysis which has led me to this result, requiring too much space to be inserted here, for the present I shall only offer a general consideration, which appears to me suited to elucidate the subject, and the truth of which may be easily seen, viz. that the formation of general equations of equilibrium of a thread, the elements of which are attracted by any forces, either internal or external, must depend on those forces, and on the mutual actions of the elements; and lastly, on the positions and on the lengths of the elements themselves, *all these elements being supposed actually in equilibrium:* from whence it follows, that the property of the elements in question to change or not in length *from the state of equilibrium* cannot at all influence the form of these general equations, which, consequently, must remain the same for elements of invariable length, as for extensible or contractile elements (which, however, does not prevent the property in question from exercising a real influence on the final results of the calculation, by the modification which it occasions in the arbitrary contents introduced in the ulterior development of the general equations). As to the tension of the thread in equilibrium, of which the expression enters into the equations of the particular case now before us,—it is clear that it is immediately derived from the forces and the mutual actions of which we have spoken, the expression of it being nothing else than that of one or other of the two parts, with contrary signs, of which the aggregate of all the forces affecting a given element of the thread in the direction of its length, is composed.

I shall add, that by proper reductions the (ii.) readily lead to the value of the tension λ

$$\frac{(A+\int Xdm)dx+(B+\int Ydm)dy+(C+\int Zdm)dz}{ds}$$

which, as is manifest, is exactly the same in the case of *a thread perfectly flexible* of an invariable or variable length; a remark which appears to me interesting, and is not found in the works of Lagrange.

His error is derived from the very specious supposition, that, in the case of elements of invariable length, the variation of *ds* may be suppressed in the expression of δe, which, although true, when only the absolute value of $\delta\epsilon$ is in question, is not allowable in forming what the author calls the *general equation of equilibrium*, into which δds should enter in every case, of which the author gives an example in the case of a thread perfectly flexible of an invariable or variable length.—*Astronomische Nachrichte*, 155.

13. *Use of Plumbago instead of Oil in Clocks and Chronometers.*—It is well known that the gradual change of oil, when applied as a lubricating medium to those parts where friction takes place in clocks, watches, and other fine mechanical arrangements, has induced numerous persons to endeavour so far to purify the oil as to prevent or retard the injury occasioned to the going of the machine as much as possible. Mr. Hebert appears to have overcome this difficulty all at

once by discarding the oil altogether, and using instead well prepared plumbago. He first prepares the plumbago by repeatedly grinding and washing it over, by which means the gritty particles that occur, even in the best black lead, are removed, and which, if allowed to remain, would neutralize every advantage the pure plumbago is found to give. This done, the prepared substance is applied with a camel-hair pencil, either in the state of powder or mixed up with a drop or two of pure spirit of wine. It readily adheres to the surface of a steel pivot, as well as to the inside of the hole in which it runs, so that the rubbing surfaces are no longer one metal upon another, but plumbago upon plumbago. These surfaces, by their mutual action, speedily acquire a polish only inferior to that of the diamond, and then the retardation of the machine from friction is reduced almost to nothing, and wear and tear from this cause is totally prevented. An astronomical clock of Mr. Hebert's own making, of which the pivots, and holes, and teeth of the escape wheel had been covered on their rubbing parts with fine plumbago fourteen years before, was taken to pieces by a committee of the Society of Arts and examined; the surfaces of plumbago were found to be for the most part unbroken and highly polished, and neither the pivots nor sockets appeared, on examination with high magnifiers, to have undergone the slightest degree of wear.—*Trans. Soc. Arts*, xlvi p. 48.

14. *Description of a Microscopic Goniometer, by* M. Raspail.—To obtain accuracy in delineating objects from a microscope, I constructed in pasteboard a model of the following instrument: a circular plate, 5.91 inches in diameter, having a perforation 3.15, and graduated on its inner circle, was placed on the object frame, so that its centre corresponded to the focus of the microscope. Two semicircles, one with a greater diameter than the other, and concentric with the disc, are moveable upon this in opposite directions, each having a hair stretched from one extremity to the other; this hair represented the common diameter. When I wished to measure an angle, I turned these semicircles in opposite directions, until the crossing of the hairs equalled the opening of the observed angle, the apex of which was exactly placed under their point of intersection. I had then only to count the degrees on the circular disc to obtain the value of the angle, taking into account the inversion of the images. But many considerable inconveniences resulted from this operation. With a considerable magnifying power, these hairs acquire too great thickness; and, on the other side, being obliged to place the object and the apparatus in the focus, and, consequently, to bring them near to each other, I could not help brushing the object almost every time that I turned round the crossed hairs.

I then devised the following process: I placed the two hairs in the interior of the microscope; one in the eye-tube, and applied against the upper surface of the interior eye-glass; and the other in the interior of the following tube, and applied almost against the inferior surface of the same glass. These hairs were each kept in their places by means of a circle of pasteboard exactly fitted to the sides of the tubes. When I wished to measure an angle of a crystal which I saw through the glasses on the object frame, I turned in opposite directions the eye-tube and the lower tube, until the crossing of the hairs gave me an angle corresponding to the angle I observed. Now, to measure the value of this angle, I placed toward the interior diaphragm of the microscope a graduated circle, and the thickness of which occupied only a very small space round the field of view of the microscope. This circle enabled me to count the degrees comprised between the opening of the two opposite angles; I then took the mean between the sum of them, and thus had the value of the measured angle. But the graduated circle, for which I was indebted to the skill of one of my friends, was of gelatine, and the impurities of this substance almost always render the degrees traced upon it indistinct, which would not be the case with a properly graduated circle of glass. This inconvenience may be remedied by the two following methods: when the value of an angle has been ascertained, it is only necessary to unscrew the tubes of the microscope immediately beneath the apparatus, then place on the side opposite to the eye-piece a diaphragm of pasteboard, with an aperture in the centre, and looking through this, the value of the angle which had been previously ascertained may be read off. A graduated circle, which would be very expensive, need not be placed in the interior of the microscope; it is only required to place horizontally, on the external eye-piece, a great circular plate, with an opening in the centre of the same diameter as the eye-piece. Two horse-hairs are then placed at right angles as diameters of this disc; these two hairs should be stretched by weights attached to their extremities. One of these horse-hairs is to be drawn above one of the hairs which is seen in the interior of the tube until they coincide exactly, which is the case when, by the effect of diffraction, the horse-hair seems bordered on each side with a straight luminous edge, but which may be traced from one end to the other of that portion of the horse hair which is seen in the field of view. When this parallelism is obtained, by the same process the other external horse-hair is placed over the other internal one, at which time the external angle, or that obtained without the microscope, is the same as that within it. Nothing further is then to be done but to measure the required angle by placing delicately a circle graduated upon gelatine above the plate which supports the two hairs; the centre of which circle must coincide with the point of intersection of the two hairs. By this microscopic goniometer great accuracy is not to be hoped for; but for occasional purposes it will answer well.—*Annales des Sciences Naturelles.*

15. *Lightning Rods.*—In consequence of the powder magazine of Bayonne having been struck by lightning, the French Minister of War submitted certain questions to the Academy of Sciences on the subject. The conclusions of the report drawn up by the Section of Natural Philosophy of this eminent body deserve much attention.

The powder magazine of Bayonne, be it observed, was vaulted below the surface of the ground to exclude wet, and vaulted above to resist the effect of shells: so it might be compared to a hollow sphere, the interior of which would be difficultly accessible to the lightning; the comparison would be still closer, had it been cooled with hydraulic lime, so as to preclude the damp from penetrating.

i. The lightning injured the powder magazine at Bayonne, only because the lightning rod with which it was provided was badly constructed, the communication between it and the ground being insufficient.

ii. A lightning rod may be rendered fully efficient by making it penetrate deeply into water or damp earth; by burying the horizontal part of it; by covering it with ashes from an oven instead of charcoal imperfectly carbonized, and by directing a stream of water toward the subterranean part of the conductor.

iii. That for powder magazines it is prudent to erect the lightning rod on a mast beside them.

iv. That in the case of magazines vaulted above and below, like that of Bayonne, and from which masses of metal of any considerable size are excluded*, it is unnecessary to employ lightning rods.

v. But in the case of magazines not so vaulted, like those for temporary purposes, prudence requires the use of a lightning rod attached to a mast.

§ II. CHEMICAL SCIENCE.

1. *On the influence of Liquefaction on the Volume and Dilatibility of certain Bodies, by* G. A. Erman.—These investigations by M. Erman have been made only upon two bodies; the first an alloy of two parts of bismuth, one of lead, and one of tin; and the second, phosphorus. Water is the only substance which had previously been examined relative to this point of molecular philosophy.

The changes in volume were ascertained by hydrostatic weighings; those of the alloy being taken in olive oil, those of the phosphorus in water. As the process required the absolute dilatation of these two fluids to be accurately known, experiments were first set on foot with regard to olive oil, for the purpose of ascertaining the expression of its true dilatation: that of water had been previously ascertained.

Numerous experiments with the alloy at different temperatures were then made; and ultimately, the results expressed graphically, in the manner practised by meteorologists, by a line bounding the abscisses formed by the temperatures expressed in degrees of Reaumur's scale; the ordinates, the corresponding changes in volume, in parts of the primitive volume at zero. The following are consequences drawn from the simple inspection of the figure: the degrees are of Reaumur's scale.

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* The commission remarks that the ordinary iron work of a building, hinges, locks, cramp irons, staples, need not occasion any fears.

From the temperature of 0° to about 35°, the changes in volume are nearly proportional to the temperature; at 35° is the *maximum* of volume; above 35° a rapid contraction comes on, which gradually diminishes to about 55°, when a change in the curve indicates the *minimum* volume. Dilatation then commences slowly, but gradually increases to 75°, when the metal fuzes; between 75° and 80°, the dilatation is considerable, but afterwards becomes the same as that observed before the irregularities commenced, *i. e.* between 0° and 35°. Calculating backwards, from 80° to 0° upon the dilatation given above 80°, it gives the same volume for the metal as is obtained by actual observation; so that it may be concluded the oscillation of volume between 35° and 75° has no influence upon the final volume after liquefaction. One very singular fact is, that the volume at the liquifying point is the same with the maximum occuring at the 35th degree.

The anomaly in the expansion of the solid alloy may be observed in the following manner:—Blow a thermometer bulb in the middle of a tube; heat it to 60°; then fill it with the fuzed alloy, by dipping one end in, and drawing out the air with the mouth at the other. Allow the whole to cool *very gradually*. Some time after the solidification of the metal, the tube will be seen to be broken to pieces by a number of small cracks. By allowing all this to take place in water, the temperature, when the glass broke, was found to be 40°. This is explained, if it be considered that at 75° the metal became solid; and that after contracting, it would at 35° resume the bulk it had at 75°; but considering the contraction of the glass between 75° and 35°, it is evident that, as the temperature approached the latter, the diameter of the metallic mass would be greater than that of the glass vessel containing it, and would consequently cause its fracture.

The dilatation of phosphorus was observed between 0° and 70°, in a similar manner as with the alloy. The results were, that solid phosphorus expanded in proportion to the temperature, with the exception of a few irregularities, probably due to imperfect observation; that liquefaction produced a sudden expansion, independent of temperature; that liquid phosphorus dilates much more rapidly than solid phosphorus; but that the dilatation is sensibly in proportion to the temperature.

Comparative Table as to Water, the Alloy, and Phosphorus.

WATER.	ALLOY.	PHOSPHORUS.
1. Dilates by solidification.	1. Condenses by solidification.	1. Condenses by solidification.
2. Expansion greater after congelation than before.	2. Expansion equal before and after solidification.	2. Expansion less after solidification than before.
3. Minimum of volume whilst liquid.	3. Minimum of volume whilst solid.	3. No minimum of volume.
4. The volumes of liquid which would be indicated by a supposed continuance of the dilatation of the solid are *greater* than those given by observation,	4. The volumes of liquid here indicated in a similar way, are the *same* as those given by observation.	4. The volumes of liquid here indicated in a similar way, are less than those given by observation.

The results of 2 and 4 strikingly support the atomic theory of dilatation. If the change in volume of a body for a unit of temperature is directly as some whole power of the distance of the molecules, then one consequence would be, the impossibility of equality between the successive changes of volumes. There is nothing, however, which leads us to suppose that the second differences of the volumes may not be so small as to escape observation, between such brief limits as those bounding the experiments. But if, by any cause, a sudden change of volume, independent of temperature, occurs, the theory requires that the increments of volume by change of temperature, before and after this sudden change in bulk, should differ; and this the comparison above confirms. Looking at 2 and 4, it will be seen that liquid phosphorus dilates more than solid phosphorus; and also that by liquefaction the volume is increased. Dismissing the oscillation in the volume of the alloy between 35° and 75°, which probably depends upon other causes than temperature, the expansion is equal before and after liquefaction, which corresponds with the theoretical conclusion (4). Again, the expansion of water is greater than that of ice; and the volumes acquired by change of state are in the sense required by theory.

It will be highly interesting to ascertain whether, for other bodies also, the greatest expansibility is always for that state in which the greatest volume is acquired.—*Ann. de Chimie*, xl. 197.

2. *On the Shock felt by Animals on the cessation of an Electric Current passing through them.*—The peculiar sensation felt by animals when they cease to form the communication between the poles of a voltaic pile, and several other physiological facts produced by electricity, have been carefully examined by M. Marianini, in a memoir first read to the *Académie de Roveredo*. The conclusions which he has himself drawn from his researches are as follows:—

i. The received principles of the voltaic pile do not authorise the conclusion that there is any reflux of electricity in it at the moment of interrupting the circuit. ii. Though such a reflux should take place, the shock felt by the animal, on ceasing to form part of the circuit, could not be attributed to it. iii. The two kinds of contractions produced in muscles by electricity, namely, the idiopathic and the sympathetic, require to be distinguished from each other in this: that the first take place, whatever be the direction in which the current penetrates the muscles; and the second only when the current pervades the nerves in the direction of their ramifications. iv. The agitation felt by animals when they cease to form part of the voltaic circuit is occasioned by the electricity, when it moves through the nerves in a direction the contrary of their ramifications, producing a shock at the instant when it *ceases* to penetrate, and not when the circulation is established. v. When the electric fluid penetrates the nerves in a contrary direction to their ramification, instead of occasioning a contraction, it produces a sensation. vi. An animal feels a sensation at the moment when an electric current, which runs through the nerves in the direction of their ramification, is interrupted.—*Ann. de Chimie*, xl. 225.

3. *Conductibility conferred by Water.*—In the second volume of this series, p. 465, we noticed the curious fact observed by M. de la Rive, that a fluid (bromine) having no conducting powers for electricity, was competent, when taken into solution by water, very much to increase the conducting powers of the latter. M. de la Rive has added another to the curious facts of that kind there referred to. Fluid sulphurous acid he finds to be a substance which does not conduct voltaic electricity. The platina wire of the voltaic pile, when plunged into it, allowed of no transference of electricity from one to the other; but so soon as a little water was added to the acid, then the current passed, and action immediately appeared. The sulphur of the acid, and the hydrogen of the water, went to the negative pole, and the oxygen of both to the positive pole; and the galvanometer was now influenced by the current, which passed through it in the ordinary manner.—*Bib. Univ.* xl. 205.

4. *Conducting Power of Mercury in the Fluid and Solid State.*—The intense cold produced by the free evaporation of liquid sulphurous acid is such, that a portion of mercury, equal to the size of a small nut, is readily frozen by it even in the open atmosphere, and retained in that state for several minutes. M. de la Rive has taken advantage of this process to institute a comparison between the conducting powers of mercury for electricity in the fluid and in the solid state. Two similar globules were placed each between two points of platina, and their exactly equal conducting power ascertained by a double galvanometer: all being then arranged so that the current of electricity experimented with should be divided equally between them, one of them was frozen by sulphurous acid, when it immediately became a better conductor than before. It appeared, therefore, that the congelation of the mercury very sensibly augmented its conducting power; and M. de la Rive asks whether this phenomenon may not be connected with the contraction which the metal is known to undergo at the moment of solidification.—*Bull. Univ.* xl. 203.

Query. How much of the effect may be due to the mere depression of temperature of the mercury and neighbouring platina wires, which Sir H. Davy has shewn increases the conducting power of *all* the metals, even without change of state?—Ed.

5. *Coloured Blow-pipe Flame; its use as a Test, by* M. Buzengeiger.—This test depends upon the colour given to the blue part of a blow-pipe flame by the introduction of different substances. It is necessary that the conical blue flame, and the blue vapour surrounding it, should be distinctly seen; for which purpose the wick is to be cut obliquely, the higher part placed on the right hand, and the wick divided for the stream of air from the point of the blow-pipe. The oil used should be such as has not been purified by sul-

phuric acid, for then it always retains a little acid, and chars the cotton. The wick should be of unbleached cotton: that which is bleached often contains a little lime, which affects the colour of the flame. The experiments are best made out of ordinary day-light. The piece of substance to be tried should be supported in platina forceps; and the blue flame being well defined, it should be introduced from below, upwards, into the external vapour just before the blue point. The form of the fragment may be wedge-shaped, acicular, or scaly. If the matter be in powder, it may be mixed into a paste in the hand, extended on charcoal, moulded into form, and then dried by a sufficient heat. When the trial piece is first introduced into the blue vapour, the latter immediately becomes of a reddish yellow colour, varying with the substance; this gradually diminishes and disappears, and then the blue vapour which bathes the body is either unaltered and scarcely visible, or else it acquires a colour according to the nature of the substance exposed to its action.

Three substances produce a red colour: strontia, lime, and lithia. Carbonate and sulphate of strontia both produce it. The mixture of baryta makes the colour disappear. Lime gives a less intense colour than strontia. Impure limestones and dolomites produce little or no colour; fluorspar, an intense colour; the sulphate, a feeble colour; the phosphate and borate none. Lithia produced a fine purple red colour, which soon disappeared; petalite produced a very feeble tint.

Blue colours are produced by arsenic, antimony, and lead.

Green colours are produced by boracic acid, baryta, and oxide of copper. Pure boracic acid yields a fine green. Borate of lime, datholite, and botryolite, produce a paler green. Borax gives a red atmosphere, unless it be first moistened with sulphuric acid. The addition of Turner's flux was not found to increase the effect, the substances giving it as well without. All barytic minerals colour the flame green. Most of the copper minerals gave a fine green colour, even though only a small quantity of copper was present. Plumbiferous minerals, containing a little copper, gave a blue flame with a green extremity.—*Ann. des Mines*, v. 36.

6. *Gas-burners.*—The effects produced upon an Argand gas flame, by varying the size of the air and gas apertures, the form of the glass, &c. is well known, but it is not so well known that the method which gives apparently the brightest light is not always that which really gives the most light for a given volume of gas consumed. From various trials, always made in reference to the quantity of gas burnt, Mr. Lowry concludes that the greatest effect was produced when the holes were numerous, and rather large than small, the central aperture narrow, and the glass near the flame; the outer aperture being in such proportion to the inner as to keep the flame cylindrical. This construction, however, when carried to the extreme, is attended with some practical disadvantages. Burners being often placed in exposed situations, the least motion of the air brings the flame in contact with the glass in such a way as to pro-

duce smoke, and the glass being intensely heated is more liable to be broken. I found it answered the purpose fully as well to enlarge the air aperture, making the glass chimney rather wider and shorter, reducing, in this manner, the speed of the air through it. Experience has shewn, at Dumfries and elsewhere, that such burners answer the purpose of requiring less gas than other burners, and giving at the same time as brilliant, and perhaps a more beautiful flame.—*Phil. Mag.* N. S. v. 375.

7. *Preparation of Compounds of Chlorine at Low Temperatures.*—Considering the change of form and condensation which occurs when chlorine and water are in contact at temperatures below 40° Fahrenheit, M. Coulier was induced to arrange his apparatus for the preparation of certain chlorine solutions, so that in winter the vessels should be exposed to the air; and he states that, by attention to this particular point of temperature, great advantages are obtained. This must evidently, also, be the case in the manufacture of chloride of lime, or bleaching powder; and, though the combinations of the chlorine itself tend to raise the temperature, yet still many degrees would, in most cases, be found entirely under command.

8. *On Boyle's Fuming Liquor.*—The theory of the formation of this well-known substance has recently been examined by M. Gay Lussac. Two parts of sal-ammoniac, two parts of anhydrous lime, and one part of sulphur, produced no azote when distilled together, but evolved at first pure ammonia, and then hydrosulphuret of ammonia in white crystals, which ultimately dissolved in the fuming liquid product. The residue in the retort consisted of chloride and sulphuret of calcium, and sulphate of lime. No hyposulphate or sulphate of lime was or could be present, as a dull red heat had been applied. Hence it is evident that the ammonia had given up no part of its hydrogen to the sulphur; no azoturet of calcium had been formed.

The hydrogen necessary may be given either by the muriatic acid, or by the water formed by its hydrogen with the oxygen of the lime: the former supposition is the most natural, but at the same time it can be shewn that water does, in certain circumstances, render up hydrogen in the production of Boyle's fuming liquor; for, on using the sulphate or phosphate of ammonia in place of the muriate, the same substance is obtained, and without the disengagement of azote. A still more direct proof is, that, on adding water and sulphur to the sulphuret of calcium, and distilling, sulphuretted hydrogen was obtained: again, by heating moistened sulphuret of barium, or passing steam over the ignited sulphuret, sulphuretted hydrogen was again obtained. This is, because that sulphuret contains three proportions of sulphur, whereas the sulphuret of calcium contains only two, and therefore requires an extra addition of sulphur.

From all these facts, it is evident that, in preparing Boyle's fuming liquor, one part of the sulphuretted hydrogen is produced by the hydrogen of the muriatic acid in the sal-ammoniac; but that the water, formed by the action of this acid and the lime at a low temperature, may afterwards react upon the mixture of sulphur and sulphuret of calcium, producing sulphate of lime and sulphuretted hydrogen. It follows, also, that the muriate may be replaced by other ammoniacal salts, provided that they are hydrated, or that water is purposely caused to be present.—*Ann. de Chim.* xl., 302.

9. *Bromide of Carbon*, &c.—Some time since M. Serullas described certain new hydriodides of carbon*. These M. Mitscherlich has shewn to be true iodides of carbon†; which result is confirmed by M. Serullas, who has since then, also, published an account of a bromide of carbon which he had previously supposed to be a hydrobromide. This bromide very much resembles the protiodide of carbon. They are both heavier than water—have the same appearance at first under its surface; the same etherial and penetrating odour and sweet taste—are both liquid, and both become colourless by washing with a solution of potash, for the purpose of removing impurities.

To form this substance, two parts of bromine are to be added to one part of periodide of carbon; just enough of solution of alkali is to be added to make the iodine set free, disappear; the liquid bromide of carbon, which will appear at the bottom of the solution, is to be separated by a funnel, or otherwise (but without washing with water), and allowed to stand until it has become quite clear: during this time a quantity of iodate of potash, in crystals, will rise to the surface —the clear fluid beneath is to be withdrawn, and put into a weak solution of potash, for the purpose of decomposing a little protiodide of carbon formed at the same time; a little bromide is also decomposed, but that which remains is soon left in a pure state.

The principal properties of this bromide, and the differences between it and the iodide, are as follows:—The first becomes solid, hard, and crystalline, at 32° F., and remains solid up to 43° F.; the latter remains fluid at the lowest temperatures. The first, heated in a spirit flame, gives red vapours—the latter, violet vapours; neither burnt with flame; but the fluid hydrocarburet of bromine does burn with flame. Neither of the two appears to act upon water, but a little alkali added, causes their decomposition slowly.

The composition of the iodides of carbon is given as follows:—

	Protiodide.		Periodide.	
Iodine . . .	1 prop.	.99528 . . .	3 prop.	2,98584
Carbon . . .	1 ,,	.00462 . . .	2 ,,	·00924

Ann. de Chimie, xxxix. 225.

10. *Hyposulphuric Acid.*—Dr. Heeren prepares hyposulphuric acid, nearly in the manner of Gay Lussac and Welter, by passing a current of sulphurous acid through a mixture of finely-divided per-

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* Quarterly Journal, xv. 297.

† Quarterly Journal, N. S. iii. 482.

oxide of manganese and water; but afterwards, to separate the sulphuric acid and oxide of manganese in solution, he uses sulphuret of barium, instead of baryta water; because the latter does not completely separate the oxide of manganese. To remove the excess of sulphuret of barium, he passes carbonic acid through, applies heat and filters; the fluid, sufficiently concentrated, yields pure crystals of hyposulphate of baryta. Being decomposed by sulphuric acid, the hyposulphuric acid is obtained pure. To obtain the largest quantity of this acid, the peroxide of manganese should contain no deutoxide; should be in exceedingly fine powder; and the temperature retained as low as possible. Anhydrous liquid sulphurous acid has no action on the peroxide of manganese.

Many of the hyposulphates were prepared by M. Heeren, sometimes directly, sometimes by double decomposition from the hyposulphates of baryta and lime. The following are briefly the characters of some of the salts:—

Potash—fine crystals; anhydrous; bitter; unchanged in air; insoluble in alcohol; soluble in 1.58 of boiling water, and in 26.5 of water at 60°.

Soda—large quadrangular prisms; bitter; unchanged in air; contains 15.54 per cent. of water; soluble in 1.1 water at 212° Fahr., in 2.1 of water at 60°.

Ammonia—difficultly crystallizable; cool taste; unaltered in air; dissolves in less than 1 of water; by heat loses water, and is then decomposed; contains 18.44 per cent. of water.

Baryta—two kinds of crystals; 10.78 per cent. water; bitter and astringent; unchanged in air; decrepitates by heat; soluble in 1.1 boiling water, and in 4.04 of water at 64°.

Strontian—large hexagonal tables; 22.10 per cent. of water; bitter; unchanged in air; not so soluble as the last.

Lime—in appearance resembles the last; bitter; 26.24 per cent. of water; dissolves in 0.8 boiling water, and in 2.46 of water at 56° F.

Magnesia—hexagonal prisms; unchangeable in air; very bitter; fusible; 37.69 per cent. water; very soluble.

The metallic oxides all form salts with this acid; all the salts, as Gay Lussac has shewn, are soluble in water, and insoluble in alcohol. —*Ann. de Chimie*, x. 30.

11. *Crystallized Hydrated Sulphurous Acid.*—M. A. de la Rive, whilst experimenting upon the liquefaction of sulphurous acid by cold, remarked the formation of crystals in several cases, and especially in the first cooled vessel into which the gas had passed, and being induced to examine them, found them to be a hydrate similar in its nature to the well known hydrate of chlorine. The crystals are beautifully white, have an acid, cool, agreeable taste, and occur in thin plates. They remain solid at temperatures near 40° F., but lose part of the sulphurous acid; at a little higher temperature they lose nearly the whole, and become water. Five parts of the crystals gave nearly one part of gas and four parts of pure water, a result which, if

brought into proportionals, will give a composition for these crystals nearly the same as that assigned by Mr. Faraday to the analogous crystals of chlorine. M. de la Rive thinks it probable from analogy that ammonia and sulphuretted hydrogen may be capable of forming similar compounds with water at low temperatures.

When water is frozen by bringing liquid sulphurous acid in contact with it, a quantity of this compound is always formed; and when sulphurous acid is allowed to evaporate in a watchglass, the dew of crystals left at last is a similar substance.—*Bib. Univ.* xl. 200.

12. *On Potassium and Sodium.*—M. Serullas remarks, that a piece of potassium put upon a bath of mercury gradually amalgamates, acquiring a rotatory motion due to its action upon the water in the atmosphere which evolves hydrogen. In dry air the amalgamation takes place without motion. But if pieces of sodium be thrown upon mercury, they are again thrown off with a small explosion, accompanied with light and heat. On the other hand, potassium burns on the surface of water, whilst sodium decomposes it, without producing combustion, so that the phenomena produced by the metals with the two fluids are the reverse of each other.

The effects on water are of course due to the superior temperature acquired by the potassium, occasioning inflammation, whilst that obtained by the sodium is not sufficient for the purpose; but if a solution of gum arabic be used, not too dense nor too thin, then the sodium fires, because the fragments, being retained at one point, become sufficiently heated, ignite with a yellow flame, and then move over the surface of the fluid like potassium. If sodium be fixed upon a bad conductor of heat, as wood, then a drop of water will fire it; but if it be placed upon glass or porcelain, then the effect will not take place; the abstraction of heat in these cases, as well as in that where a surface of pure water is used, is too rapid to allow of the sufficient elevation of temperature.—*Ann. de Chimie*, xl. 327.

13. *Separation of Manganese and Iron, by* M. Lassaigne.—This process will not serve analytical purposes, but will supply a pure oxide of manganese; it is founded upon the insolubility of the oxalate of manganese, and solubility of oxalate of iron; but when the latter salt is present, a little of the former enters into solution. Peroxide of manganese is to be acted upon by weak muriatic acid to remove carbonates, then mixed with four or five times its weight of oil of vitriol, heated and evaporated to dryness. After this, eight or ten times its weight of water is to be added; this produces a solution of protosulphate of manganese containing iron and also copper: the solution is to be acidulated by sulphuric acid, the copper thrown down by sulphuretted hydrogen, the clear liquor boiled to drive off the excess of sulphuretted hydrogen, and then precipitated by carbonate of soda. The yellow-white mixture of carbonates of manganese and iron is to be well washed, and then digested in a hot solution of oxalic

acid oxalate of manganese in a fine white insoluble powder is formed, from which the soluble oxalate of iron is to be removed by washing. The purified oxalate, being heated in close vessels, gives off a mixture of carbonic oxide and carbonic acid, and a pure greenish grey protoxide of manganese remains.—*Ann. de Chimie*, xl. 329.

14. *Preparation of Protoxide of Chromium, by* M. Frick.—This process is intended for large quantities. The mixture of chromate of iron and nitrate of potash is to be heated to redness as usual, the mass to be washed with water, and the solution, which is often green, evaporated very considerably, in an iron pan. Upon cooling, the solution is to be decanted into glass vessels, and the insoluble residue washed and rejected. The clear yellow liquor obtained is to be boiled in an iron vessel, and flowers of sulphur added so long as they occasion a green precipitate; this precipitate is to be well washed, dissolved in dilute sulphuric acid by means of heat, then precipitated by carbonate of soda, and the precipitate washed and dried.—*Ann. der Physik*, 1828.

15. *Reduction of Sulphuret of Arsenic.*—In testing for arsenic there is often occasion to reduce very small quantities of the sulphuret in such a manner as to develope most effectually and surely the metallic nature of the base. M. Berzelius's process is now well known; the following is M. Liebeg's: the sulphuret formed by the addition of sulphuretted hydrogen to the acid solution, containing arsenic in the usual way, is to be collected, well dried, and put into a small glass tube, drawn out at the extremity so as not to be larger than a large needle, and then closed; a layer, two or three lines in thickness, of carbonized tartrate of lime is then to be put over it, and the whole heated by the blowpipe; the upper part first and afterwards the lower. The arsenic is reduced, and forms a brilliant film above the charge of materials, even when the quantity of sulphuret is not more than $\frac{1}{500}$ of a grain.—*Ann. der Physik*, 1828.

16. *Rhodium Salts*, according to Berzelius.—

Chloride of Rhodium and Potassium.

Chloride of Potassium	41.50
Chlorine	29.53
Rhodium	28.97

Chloride of Rhodium and Sodium.

Chloride of Sodium	45.55
Chlorine	27.48
Rhodium	26.97

A Hydrated Oxide of Rhodium.

Rhodium	75.9
Oxygen	17.5
Water	6.6

17. *Braconnot's Indelible Ink, or Dye.*—Whilst engaged in experiments on the production of deep brown colours for dyeing, at an economical price, MM. Braconnot and Parisot were led to examine the nature of the products obtained by the calcination of various organic substances with potash. These results were found to vary with the substances used: thus, when matters containing but little azote were employed, only little colour could be fixed upon fabrics, and that easily removable by alkaline solutions ; but when such substances as flesh, skin, and horn were employed, more permanent and deeper colours were obtained. Suspecting that this difference might be due in part to the sulphur present in these bodies, sulphur was purposely added, and then a deep chesnut brown dye was procured, more permanent than any other of the kind known in the arts. This immediately led to the preparation of an indelible ink, which was prepared in the following manner :—20 parts of Dantzic potash were dissolved in boiling water, and 10 parts of tanned leather parings, in small pieces, with 5 parts of sublimed sulphur, added ; the whole was boiled to dryness in an iron vessel, then heated more strongly with continual agitation, but avoiding ignition, until the whole became soft ; then a proper quantity of water was gradually added, and the whole filtered through a cloth. In this way a very dark coloured liquor was procured, which may be preserved for any period in close vessels, and which ought to be retained in well corked vessels, constantly excluded from the air ; this presents no difficulty to its use, for a pen full is sufficient to write a couple of quarto pages. It flows much more freely than ordinary ink, does not embarrass the pen with insoluble matter, and resists chemical agents in such a way as to merit the title of indelible ink.

Paper written with it, when subjected to the action of a boiling solution of potash, or strong sulphuric acid, or strong nitric acid, was nearly destroyed, whilst the characters remained unimpaired. Being first immersed in a mixture of chloride of lime and muriatic acid, and then for twenty-four hours in caustic potash solution, which was ultimately boiled to dryness, still the letters were very distinct. Weaker actions than these could of course do no harm, stronger could hardly be devised.

It is presumed that the same preparation will be found very useful in giving chesnut browns, more or less deep in colour, to cotton, hemp, flax, and silk ; or to assist other substances in producing colour. Substances tinted by a salt of iron take a deeper tint from this preparation than those not so prepared. It is also proposed to use it upon linen as an indelible ink.—*Ann. de Chimie*, xl. 221.

18. *Test for Vegetable and Animal Matter.*—The nitrate of silver is the test which Dr. Davy thinks to be one of the best for detecting the presence of organic matter in solution. A pure solution of this salt is not altered by the sun's rays ; but if the minutest quantity of animal or vegetable substance be dissolved in the water, the solution is discoloured ; with common distilled water, the discolouration is

strong. To prove that the cause of change assigned is the true one, it is only necessary to decant the colourless solution and expose it again to sunshine; however powerful the sun's rays, no further effect will be produced, unless a little more common distilled water be added, and then it re-appears. When used as a test for such substances, of course any chloride of silver that may be formed in consequence of the presence of muriates should be allowed to subside in the dark, and the subsidence should be complete before the fluid is decanted and exposed to light.—*Jameson's Journal.*

19. *On Starch, by* M. Guibourt.—M. Raspail shewed that starch is not a homogeneous body, but that each grain is a true vegetable organ, composed of—i. a smooth tegument or envelope, unaltered by water or acids at ordinary temperatures, and susceptible of strong colour by iodine; ii. an internal substance, soluble in cold water, liquid even in the natural state, which by evaporation loses the power of acquiring colour from iodine, and possesses the properties of gum. The colouration by iodine he thinks to be due to a volatile substance, and not to the starch itself.

The following results, by M. Guibourt, are from experiments on patatoe starch prepared by himself. When dry, the grains appeared under the microscope of all forms, from spherical to triangular; smooth, transparent, greyish at the edges; all the grains were free; insoluble in cold water, even after many hours maceration. When rubbed with a muller, they lost their whiteness and brilliancy, and sometimes adhered in humid air; a little water then made them into a paste, very hard when dry. When beaten in a mortar, they gave a mass like gum adraganth.

The entire grain was scarcely coloured by iodine, and each retained its transparence. The broken grains put upon water caused rapid currents from the emission of soluble matter, and after a little heat had been applied the broken envelopes could be readily observed. Aqueous solution of iodine being added, before the application of heat, gave a sky-blue colour to the whole, a deep blue to the portions of gelatinous appearance upon each grain; and slowly, such depth of colour to the envelopes that they appeared opaque.

These experiments shew that as to iodine, both the soluble and the insoluble part are alike, and in fact merely differ in density or slight modifications. The soluble part, after long boiling, was still colourable by iodine; being rapidly evaporated, films were formed which were not entirely soluble in water; being evaporated to dryness, and re-dissolved, parts were left insoluble exactly like the teguments; but in all cases, both the soluble and insoluble parts became blue with iodine, so that the colouration is not due to a volatile principle, nor is the soluble part gum. These facts, in addition to the circumstance that both the teguments and the insoluble portions above produced are both soluble if boiled in sufficient water, lead to the conclusion that they differ more in form than in nature, and that both soluble and insoluble parts consist of one immediate vegetable principle.

Then arises the question, is the amylaceous principle alike in all plants? which has hardly as yet been settled.

Starch of Corn, under the microscope, appears composed of spherical globules of various sizes; the smallest appear like points, are most numerous, and seem to be just produced; the largest are smaller than those of potatoe starch; it is quite white and dull; it gives to water a stronger gelatinous consistency, because its particles being small, a larger proportion of teguments and a less proportion of soluble matter is present. Starch paste consists of an aqueous solution of the soluble part, holding in suspension the teguments, which, swelled, in contact, and adhering together, give consistence and demi-opacity to the mass; if it be boiled for a long time in much water, the teguments become more and more divided and at last dissolve, and then the liquor does not again acquire the gelatinous consistency on cooling. In the starch of commerce a few of the globules have been broken by the mill or in fermentation, and their contents give the adherence and hardness which the masses acquire when dry. Potatoe starch, not having suffered these processes, is always pulverulent.

Arrow-root, or fecula of the *Marantha Indica*, is composed of grains larger than those of corn starch, more brilliant, and quite transparent. They are principally spherical, sometimes triangular, like those of potatoe starch, but are much smaller. Arrow-root gives less consistency to boiling water than corn starch, probably because it contains more of the soluble principle.

Cassava and *Tapioca* are obtained from the root of the *Jatropha Manihot*, and differ only in the first being dried in the free air, the latter on hot plates of iron—hence its agglomerated form. All the grains of cassava are spherical, remarkably equal in size, and smaller than those either of arrow-root or starch; by these circumstances it may be distinguished from all other fecula. *Tapioca* is in lumps, formed of broken and aggregated grains; put into *cold* water, part dissolves, but not the whole; the unbroken grains resemble those of cassava.

Sago is the fecula of the *Sagus Farinaria*, and is in small round, hard, white masses. The microscope shews these to be composed of adhering entire grains, resembling those of potato starch. Cold water dissolves nothing, but heat applied bursts the grains; the teguments are the most unchangeable in water of any found in amylaceous molecules. It does not appear that the colour of sago is due to the action of heat, but rather to some foreign substance.—*Jour. de Chimie Médicale*, v. 97.

20. *On the Resin of Indigo and Artificial Tannin, by* Dr. Buff.—When indigo is acted upon by dilute nitric acid, besides indigotic acid, there occurs a brown resinous substance, and a reddish oily viscid liquor, both acrid and bitter. Whilst examining these substances, and varying the proportions used, it was found that in proportion as weaker acid and more indigo was used, the resinous nature

of the brown matter passed off, and the quantity of the oily fluid diminished; but the latter was never quite absent, although occasionally all in solution. From this, it appeared that nitric acid was an essential part of these two substances; and this was confirmed by adding nitric acid to the brown matter, its resinous qualities increased: by adding excess of acid, it dissolved and became red; on evaporating, red oleaginous drops appeared; finally, the whole of the liquid became artificial tannin, and solidified on cooling, preserving its colour. No gas was given out; and artificial tannin was shewn to be therefore a compound of nitric acid and the resinous matter.

To determine the nature of the latter, it was boiled a number of times in water, by which a brown, friable, and insoluble substance was obtained. The mother liquor, by evaporation, gave indigotic acid and artificial tannin in the red state; and it was ultimately found that the resinous substance is an intimate mixture of the above friable matter and artificial tannin, and that the tannin consisted of the same friable substance, united to indigotic and nitric acids; so that the two bodies differed only by the proportions of their constituents, and would allow of infinite gradations coming between them, as, indeed, is shewn by the action of strong nitric acid in excess upon indigo.

The artificial tannin is but little soluble in cold water, but readily in hot water, alcohol, and nitric acid: it is soluble also in alkalies; but if the solution be rendered neutral by an acid, tannin is not precipitated, but the resin of indigo; the latter is at once transformed into the tannin by treatment with nitric acid.

The base of these substances, *i. e.* the friable matter, may be procured pure by acting upon one part of finely pulverised indigo with two parts of concentrated nitric acid, previously diluted with from fifteen to twenty parts of water. The residue, filtered, and well washed with boiling water, is to be put into a solution of an alkaline carbonate, which will leave the undecomposed indigo; and then the solution, being precipitated by an acid, will throw down a precipitate, which, washed with alcohol, is the pure substance. This substance is insipid, brown, friable, insoluble in water or alcohol, swelling and decomposing by heat, at the same time evolving the odour of cyanogen. It is not decomposed by nitric acid, but forms with it artificial tannin, from which the original weight of brown matter may be obtained. Chlorine does not act upon it.

The quantity of this substance obtained from indigo varies. Ordinary indigo yields much of it; fine India indigo much less. That which yields the most of this substance, yields the least of indigotic acid. From this it might be concluded that it is formed by the extraneous matter in the indigo; and in some imperfect trials, sublimed indigo gave little or none of it, but much indigotic acid.

Theoretical views lead to the supposition that pure indigo is not decomposed, but only oxygenated by nitric acid, without the formation of any other products. Thus, according to the experiments of Royer, Dumas, and Crum, and neglecting the hydrogen, it has the following composition, speaking in definite proportionals:—Two of azote, fifteen of carbon, and two of oxygen. Indigotic acid consists (according to Dr. Buff) of two azote, fifteen carbon, and ten oxygen. Carbazotic acid (according to Liebeg) contains four azote, ten carbon, and ten oxygen; and, finally, white or deoxygenated indigo contains two azote, and fifteen oxygen. Supposing, therefore, that indigo contains no hydrogen, which is probably the truth, the following is the state of things:—

	White Indigo.	Blue Indigo.	Indigotic Acid.	Carbazotic Acid.
Azote	2	2	2	4
Carbon	15	15	15	10
Oxygen	0	2	10	10

an extraordinary example of an organic substance acting with oxygen like an elementary body.—*Ann. de Chimie*, xxxix. 290.

21. *Preparation of Sugar from Starch.*—M. Weinrich says, that from one to two parts of sulphuric acid for each 100 parts of potato starch, is sufficient, if the heat applied be a few degrees above 212° F.; and also that then two or three hours are sufficient to give crystallizable sugar. He applies the heat in wooden vessels, by means of steam.

22. *Preservation of Wine Must.*—The following curious effect has been observed by M. Leuchs:—Charcoal was added to grape must, in the proportion of 100 grains to a litre (2.1 pints); or, if very much inclined to ferment, rather more charcoal was used. When the liquid had settled, and become clear and colourless, it was removed from the charcoal, and put into bottles, or casks, to be closed up and preserved. It will not enter into fermentation, even in close vessels; for the charcoal has absorbed the ferment. Nevertheless, the ferment has not lost its powers by combination with the carbon; for, if left in the must, the latter begins to ferment, but only where in contact with the former.—*Bull. Univ.* D. xi. 62.

23. *Preparation of Piperine.*—The following is M. Voget's process and result:—Sixteen ounces of coarsely powdered black pepper was digested in double its weight of water for two days, five times in succession, and the residue strongly pressed and dried. It was then digested for three days in twenty-four ounces of hot alcohol; the fluid pressed out, filtered, distilled, and ultimately evaporated to a syrup. The impure crystals of piperine deposited on cooling, were washed with ether, to remove resin, dissolved in three times their weight of hot alcohol, sixty grains of animal charcoal added, the liquid filtered and evaporated spontaneously, when 110 grains of pale yellow crystals of piperine were obtained. On repeating this process with the residue of the maceration, seventy grains more were obtained.

According to other experiments, it appears that the green acrid resin in the pepper, which in the above process was dissolved by the ether, is the active and febrifuge principle in pepper, rather than the insipid piperine.—*Brande's Archives. See vol.* iv. *p.* 221.

24. *Principle from the Volatile Oil of Orange Flowers.*—According to M. Plisson, if alcohol of s. g. .842, be added to the recent essential oil of orange-flowers, until all further precipitation ceases, the substance thrown down be washed with alcohol, dissolved in sulphuric ether, and crystallized, it will be found to be a peculiar substance, crystallizable, white, nacreous, without odour or taste, and without action on vegetable colours. It forms about $\frac{1}{100}$ of the recent oil, but diminishes with age. At 131° F. it is a fluid, and on cooling again, becomes a solid, like wax; its specific gravity is then .913. When carefully heated, it volatilizes without change; more hastily heated, it is decomposed, more or less. It is insoluble in water, but soluble in sixty parts of hot alcohol. Nitric and muriatic acid appear to exert no action upon it. Hot sulphuric acid chars it. It is not saponified by alkalies. This substance does not appear to be an essential part of the volatile oil, but merely to have a common origin with it. M. Plisson proposes to call it *aurade.*—*Ann. de Chimie*, xl. 83.

25. *Aspartic Acid.*—The crystalline matters obtainable from the young shoots of the asparagus, the liquorice root, and the marsh-mallow, have been shewn by M. Plisson to be identical; and have been described under the name of *asparagine.* When brought into contact with the hydrated oxide of lead, it produces a new insoluble salt, which being decomposed by sulphuretted hydrogen, supplies the *aspartic acid.* This has lately been examined by M. Plisson with more care than upon a former occasion, and the following are its properties:—

It crystallizes from water in a fine brilliant powder, consisting of long quadrangular prisms, with diedral summits, transparent and colourless. It is inodorous, slightly acid to the taste, and reddens litmus. It dissolves in 128 parts of water, at 47° F.; but is more soluble in hot water. It is insoluble in alcohol; has a specific gravity of 1.873. It is decomposed by heat, producing ammonia and Prussic acid. Hot sulphuric acid destroys it; hot nitric acid has very little effect upon it. It displaces carbonic acid. It has the power, by long ebullition, of converting starch into sugar: this power belongs also to the kinic acid.

Many *aspartates* have been formed: those which have a mineral alkali base are decomposed by heat into ammonia, hydrocyanic acid, cyanide of the metal *, &c. All the soluble aspartates have the flavour of meat juice in a remarkable manner; this flavour is most pure in neutral alkaline or earthy salts; is followed by a styptic taste in the metallic salts; and is overpowered in the salts containing vegeto-alkaline bases. Its equivalent number appears to be about 135.26; oxygen being 8.—*Ann. de Chimie*, xl. 309.

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* Where has the oxygen of the base gone to?—Ed.

26. *On some Chlorides of Platina, by* G. Magnus.—The protochloride is obtained by heating the perchloride to the temperature of melting lead. If it be pure, it is insoluble in water; but if the perchloride from which it has been obtained was not sufficiently heated, the residue dissolves in water, with such a dark brown colour, that the liquid, although clear, appears entirely opaque: it is a solution of the protochloride in the perchloride. On evaporating, a brown powder is precipitated, which does not dissolve again in water, when the water in which it was suspended is removed; but on adding this to water, the brown precipitate is redissolved by heat. This precipitate is distinguished from the protochloride obtained by heat, by its colour, and particularly by its greater solubility in muriatic acid; nevertheless, the composition is precisely the same, for, decomposed by hydrogen, it leaves the same quantity of metal, 26.41 per cent.

The proportion of the protochloride dissolved by the perchloride depends on the dilution of the solution. After evaporation to dryness, the residue may be redissolved by continued ebullition; but if cold water be employed, only a very small quantity of the protochloride dissolves in the perchloride. By repeating the same series of operations frequently, the solution becomes more and more freed from the protochloride it contained. The brown chloride of platina dissolves in muriatic acid, with a particular red colour, and so plentifully, that it might be imagined a combination was formed similar to those that Boullay has pointed out between muriatic acid or hydriodic acid, and other chlorides and metallic iodides.

If chloride of potassium be added to this solution, an acid salt is obtained in prismatic crystals, which, analyzed by hydrogen, affords,

Platina	46.74
Chlorine	17.47
Chloride of Potassium . . .	35.79

or one atom of protochloride of platina, and one atom of chloride of potassium. This salt is very soluble in water, but not in alcohol. The double salt of potassa, and that of ammonia, is obtained in the same manner. The first does not crystallize in water, and is very soluble in alcohol; the second crystallizes, on the contrary, very easily in deep red crystals.

If to a solution of the protochloride in muriatic acid ammonia be added in excess, at the end of some time there is precipitated a green salt, crystalline, insoluble in water, alcohol, and even muriatic acid. It is composed of one atom of protochloride of platina, and one atom of ammonia, or of

Platina	65.21
Chlorine	23.42
Ammonia	11.34

It has been analyzed, by decomposing it with carbonate of soda.—*Ann. de Chimie, Janvier.*

§ III. NATURAL HISTORY, &c.

1. *Effect of Chlorine as an Antidote to Hydrocyanic Acid.*—The following is abstracted from a letter by M. Dauvergne to M. Gay Lussac, describing an experiment made by himself and M. Siméon. Two drops of hydrocyanic acid were put into the end of a glass tube, and introduced into the lachrymal gland of a cat: contractions immediately came on, followed by strong tetanic convulsions; an abundant salivation took place, producing, through hard breathing, a thick white froth. The pulsations of the heart were quick, irregular, and extensive, as if each were the last effort of life. Inspiration was difficult and painful; expiration, frequent, prompt, and forcible. Notwithstanding this desperate state of the animal, M. Siméon was induced, from his previous knowledge, to expect good effects from the use of chlorine, and therefore introduced a considerable quantity into the mouth: the salivation in consequence ceased; the respiration became easy; the circulation less forced and rapid. The animal now raised its head, which before it could not do; put out its tongue, and scented the chlorine as if it took pleasure in respiring a salutary and agreeable atmosphere. In this manner, the symptoms gradually diminished, but as yet the cat could not stand up. Being exposed to the open air for a few minutes, it voided a large quantity of fæces, gradually rose on its feet, and made a few tottering steps: this was in one hour after the poisoning. At the end of two hours, traces of the event were scarcely visible; and the next morning the cat ate, drank, and walked, as if in perfect health,—no signs of the effect of the previous day remaining.—*Ann. de Chimie*, xl. 334.

2. *Velocity of the Blood in Circulation, and Rapidity of Secretion.*—Numerous experiments on these points have been made by M. Hering, Professor at Stutgard, upon horses, into whose jugular vein was inserted a solution of hydrocyanate of potash and iron. After some time, veins were opened in other parts of the body, and the blood taken out allowed to stand until a little clear serum could be removed from it; this being placed on white paper, a few drops of a solution of sulphate of iron added, and immediately after a drop of strong muriatic acid: the hydrocyanate, if there, was immediately indicated by the blue colour. After being bled, the animals were killed, and traces of the introduced liquid sought for in the secretory organs and their products.

The solution introduced into the jugular vein of the horse runs the course of the circulation, and arrives in the jugular vein of the opposite side in from 20″ to 25″, or from 25″ to 30″. It arrives in from 23″ to 30″ in the external thoracic vein of the opposite side; in 20″ at the vena saphena major; in from 15″ to 20″ in the mesenteric artery; in from 20″ to 26″ in the maxillary artery; and in from 20″ to 25″, and from 25″ to 30″, in the metatarsal artery,—always on the opposite side to the place of injection. The velocity of the motion does not appear to be increased in the ratio of the pulsations of the heart; for in cases where the pulse was 60, 36 to 44, and 48 to 52, per minute, the results were the same. In one case the circulation was a few seconds slower.

The salt used was promptly secreted by the serous membranes, but only in small quantities, and that in the direct ratio of their distance from the heart. None was ever found in the cerebral cavities. The mucous membranes caused its secretion less quickly; but a few minutes sufficed to shew its presence at their free surfaces, and soon after at their other surfaces. The mucous membrane of the right half of the stomach secretes more promptly than that of the intestines, and the latter more rapidly than the surface of the lungs. The mucous surfaces, covered with an epithelium, (as the walls of the mouth, the pharynx, the left half of the stomach, in the horse,) gave no traces of the ferro-prussiate.

It was found but with difficulty in the liver, spleen, thyroid gland, &c., because of their dark colour. The salivary glands separated much of it. The kidneys, also, act powerfully upon it. It was found, after one minute, in the cortical and tubular parts, and in the pelvis. The small blood-vessels of the kidneys gave indications of it before the large ones, from whence it would appear either that the circulation is slower in the former, or that the salt commences its separation before hand.

The saline solution adheres in some cases to the sides of the blood-vessels, and is then easily discoverable, but more frequently it does not. The cause of the difference is not known. The shortest time which it takes to reach the thoracic duct is not known: a minute was sufficient in one case, and from two to five in others. It appears a little later in the lymphatic ganglia, from which it is supposed that a direct communication between the arteries and lymphatic vessels exists.

The foreign substance introduced is quickly ejected by the secretory organs, especially the kidneys. In from five to eight hours, traces of it had disappeared from the secretions, and in twenty-four hours, from even the solid and every other part. The ferro-prussiate of potash produces no inconvenience to the animal.—*Zeitschrift für Physiol.* ii. 85.

3. *On the Proportion in Births of Boys to Girls.*—The following is extracted from a memoir by M. Poisson. Whilst considering the births for six successive years, he was led to remark that, first, the ratio of boys to girls was $\frac{16}{15}$ instead of $\frac{22}{21}$, as had been supposed; second, that the ratio being nearly the same for the south of France, and France entire, did not appear to depend upon climate, at least in that country; third, that, amongst illegitimate children, the ratio is less, and only $\frac{21}{20}$.

These results were published in 1823, and since then have been verified in a still greater number of births. The following is the ratio of boys to girls, legitimate and illegitimate, in all France, for ten years:—

1817	1.0720	1822	1.0628
1818	1.0644	1823	1.0621
1819	1.0642	1824	1.0659
1820	1.0642	1825	1.0703
1821	1.0685	1826	1.0614

Mean result, 1.0656.

The mean is from near ten millions of births, and differs only 1-200th from the extremes of 1817—1826: the mean of the thirty southern departments is 1.0665. The number of illegitimate children, in the above ten years, is nearly 700,000; and, in this number, the males are to the females in the ratio of 1.0484. The difference of 0.0172, between this and the great mean, is too large; and the number of births, from which it is drawn, too considerable to suppose it results from accident; and, however singular it may appear, there is great reason to believe that there is some cause existing, as to natural children, which diminishes the preponderance of male over female births. This influence is perceived even in the annual births.

The proportion between boys and girls is not the same for Paris as for the departments. From 1815 to 1827 there were born in Paris 215,000 legitimate children; the males were to the females in the ratio of 1.0408, or nearly $\frac{26}{25}$, instead of $\frac{16}{15}$. In the same time were born 122,000 illegitimate children, amongst whom the ratio was 1.0345, or about $\frac{30}{29}$, instead of $\frac{21}{20}$; from whence it is to be presumed that, in a great capital like Paris, there exists a particular cause which diminishes the preponderance of boys over girls, and acts both upon legitimate and illegitimate children.—*Ann. de Chimie*, xl. 39.

4. *Inoculation of Hydrophobia.*—Some experiments have recently been made at the Veterinary College at Alfort upon this subject. A horse, two dogs, and three sheep, were inoculated with the saliva of a sheep affected with hydrophobia, at various stages of the disease, but no symptom of the malady was produced on the animals thus experimented upon. Four months had elapsed after their inoculation when this report was made.—*Med. Journal.*

5. *Ossification of the Vitreous Humour.*—M. Krehn has lately met with that rare case, the ossification of the vitreous humour of the eye. It occurred in a man seventy years old, who died of gastritis: the preparation is placed in the Strasburgh Museum. The left eye was healthy, but the right presented the following appearance:—The globe was diminished in size, had lost its spheroidal figure, and presented the appearance of four wrinkles, or furrows, corresponding with the insertion of the recti muscles. It was heavy and hard. When a horizontal section was made from behind forward, the sclerotic was found to be very thick, particularly at its posterior part, near the entrance of the optic nerve; the instrument was soon arrested

by a hard body, filling the whole space of the eye-ball behind the crystalline lens, and consequently occupying the place of the vitreous humour. Immediately within the sclerotic was the choroide membrane, distinct, and rather thicker than natural. The retina was unchanged: the solid body within was marked by the same depression which had been observed externally. It was of a pale white colour, and was internally of a cellular texture, like the cancelli of the long bones. The crystalline was indurated, and of a yellowish-white colour: the optic nerve was wasted.—*La Clinique.*

6. *Vocal Sounds after Death.*—M. Maingault states that, in experiments made with the larynxian tube, he had occasion to observe a phenomenon noticed by M. Dutrochet, namely, that when air was forcibly thrown into the lungs of a dead infant or animal, the air, when thrown out again through the larynx, produced a sound analogous to that produced by the infant or animal in the living state.

7. *Preservation of Blood.*—Sugar refiners and others are often inconvenienced by the difficulty of obtaining blood at the time when it is required for use. M. Toursel has endeavoured in part to remove these difficulties, by proposing a method of preserving this agent for some time without injury. It consists in putting the blood into bottles, or other vessels, with very narrow mouths, and being careful to fill them up to the neck; a layer of oil, to the depth of at least half an inch, is then put upon it to cut off communication with the atmosphere, and the whole is left to itself. M. Toursel states that he has, in this manner, preserved blood, with all its physical and chemical qualities, from the 1st of December 1827, to January 1829.—*Journ. de Commerce.*

8. *Stinging of a Gnat.*—On the 25th of October last, and about two o'clock in the day, a gnat (*culex pipiens*) alighted on my forefinger. I held my hand still, and observed it. It immediately applied its proboscis to the skin, at the same time moving its antennæ and hind legs slowly up and down, which it continued to do for a few seconds, when it became apparently motionless, resting on its four front legs, with its hind ones stretched out in a line with its abdomen. I now felt a slight sensation where the proboscis was inserted, but so faint, that, had not my attention been directed to it, I probably should not have noticed it, and this I only felt for a second or two. The abdomen now began to swell gradually; the influx of the blood being clearly visible through its semitransparent skin; at the same time a clear watery fluid began to ooze from the anus, forming a round globule. It had a curious appearance to see blood flowing in at one end and water out at the other; the drop fell and another formed—the abdomen all the time getting larger and larger, and redder and redder, until the second drop fell. The abdomen had now attained full three times its natural size, with a deep blood-red hue, when, to my great vexation, the door opened, and away flew my

little toper, without appearing the least encumbered with its bloody cargo. I must further observe, the insect was altogether about one minute on my finger; that no part of the outer sheath of its proboscis was inserted beneath the skin; that I did not receive any warning of its intentions from its pipes. There was not the slightest pain, inflammation, or mark of any kind left that was perceptible.—J. A. —*Mag. of Nat. Hist.* ii. 69.

9. *Destruction of Weevils in Granaries.*—After trying all the methods of extirpating these creatures that have been proposed, M. Boode found the most effectual to be as follows:—The floors and sides of the granary are to be watered with a mixture of urine and water before the corn is stored up; this watering is to be repeated several times, the walls and floors of the granary being well swept between each operation. The success which M. Boode obtained by this process was perfect, and was the more striking, as previously he was troubled with enormous quantities of these insects.—*Bull. Univ.* D. xi. 31.

10. *Zoological Weather Glass.*—At Schwitzengen, in the post-house, we witnessed for the first time what we have since seen frequently, an amusing application of zoological knowledge, for the purpose of prognosticating the weather. Two frogs, of the species *Rana arborea*, are kept in a glass jar about eighteen inches in height and six inches in diameter, with the depth of three or four inches of water at the bottom, and a small ladder reaching to the top of the jar. On the approach of dry weather the frogs mount the ladder, but when wet weather is expected they descend into the water. These animals are of a bright green, and in their wild state here climb the trees in search of insects, and make a peculiar singing noise before rain. In the jar they get no other food than now and then a fly, one of which, we were assured, would serve a frog for a week, though it will eat from six to twelve in a day if it can get them. In catching the flies put alive into the jars the frogs display great adroitness.—*Mag. of Nat. History*, iv. 479.

11. *The Great American Bittern.*—I was much interested with an account I heard the other day of a bird, a species of heron. I believe called by Wilson, in his *Ornithology*, the Great American Bittern: but, what is very extraordinary, he omits to mention a most interesting and remarkable circumstance attending it, which is, that it has the power of emitting a light from its breast equal to the light of a common torch, which illuminates the water so as to enable it to discover its prey. As this circumstance is not mentioned by any of the naturalists, that I have ever read, I had difficulty in believing the fact, and took some trouble to ascertain the *truth*, which has been confirmed to me by several gentlemen of undoubted veracity, and especially by Mr. Franklin Peale, the proprietor of the Philadelphia Museum. (Letter from Philadelphia, Oct. 11, 1828.)—*Mag. of Nat. Hist.* ii. 64.

12. *Introduction of Coffee into Europe.*—The introduction of coffee into Europe is not quite certain, but it appears to have been first employed in Venice about 1615, at Paris in 1644, and at London in 1652. It is estimated by the Abbé Raynal that twelve millions of pounds were imported annually into Europe before the plantations in the colonies were established. The Dutch introduced it into Batavia about the year 1696; the French into Martinico in 1727, after it had been in the Isle of Bourbon in 1717; and the English into Jamaica in 1728. The cultivation of it was then introduced at Ceylon, Sumatra, and other European possessions in India. It had been propagated at Surinam in 1718. This plant has been found native at St. Domingo, in Abyssinia, at Mozambique, on the coast of Zanguebar, and in the forests of Orapu.—*Oriental Herald.*

13. *French Eggs and Apples.*—63,109,618 hen's eggs, and 14,182 bushels of apples were imported from France into England in the year 1827.

14. *Enlargement of Artichokes.*—An effectual means of increasing the size of artichokes is to split the stem into four at the base of the receptacle, and introduce two small sticks in the form of a cross. This operation has long been practised in the south of France, and for some years past in the neighbourhood of Brussels. It should not be carried into effect until the stem has attained the height it ought to have.—*Jour. d'Agriculture.*

15. *On the Preservation of Potatoes.*—Potatoes, at the depth of one foot in the ground, produce shoots near the end of spring; at the depth of two feet they appear in the middle of summer; at three feet of depth they are very short, and never come to the surface; and between three and five feet they cease to vegetate. In consequence of observing these effects several parcels of potatoes were buried in a garden at the depth of three feet and a half, and were not removed until after intervals of one and two years. They were then found without any appearance of germination, and possessing their original freshness, firmness, goodness, and taste.—*Ann. de la Soc. d'Agric.*

16. *On the action of Mushrooms upon Air and upon Water.*—All who have paid attention to vegetable physiology know that most plants, during the period of their vegetation, have the property of changing the chemical nature of the air in which they are placed. Some experiments upon mushrooms, in respect to this property, are detailed in the following paper. Up to the present moment, the vegetables in question are so little known, and differ so much from the rest of the vegetable kingdom, that every comparison between their properties, and those of ordinary vegetables, appears to me interesting.

On this subject, I have found the following passage in the Aphorisms at the end of the *Flora Fribergensis* of M. de Humboldt, published in **1793**: *Specimina agarici campestris juniora antequam pileum expilatum et annulum perruptum, diu et noctu gaz hydrogeneum emittere deprehendi;* and, in another place, *Idem in agarico androsaceo deprehendi.*

M. de Candolle, in the second volume of his French Flora, says he has found that the *sphæria digitata*, exposed under water to the sun, yields a gas in which he found seventy per cent. of hydrogen. The same savant told me that he has obtained fourteen per cent. from the *peziza nigra*, placed under the same circumstances.

I undertook the following experiments to ascertain, 1°, if mushrooms, during the period of their vegetation, really exhale particular gases, or change the nature of the air like other plants; 2°, of what nature are the gases which seem to be exhaled by mushrooms placed under water, and what may be the cause of this phenomenon.

§ i. *Action of mushrooms upon air.*

I covered, with a glass receiver, an agaricus which was only just springing from the earth. Having surrounded the whole base with a sort of unctuous lute, I made the receiver adhere in such a way, that it was impossible there could be any communication between the included and the external air. At the end of three days, the agaric had increased to nearly four times its original size. I then analyzed the air contained in the receiver to see if it had undergone any change. This experiment was repeated three times on different species of agaricus: in no case did I discover the least trace of hydrogen gas. There was found in the receiver only a very small quantity of carbonic acid gas, varying in quantity according to the species of agaricus, and even to the individual subjected to the experiment.

Mushrooms gathered from the earth, and exposed under a glass bell either to the sun or to darkness, appeared to me not to change the nature of the air in which they were, and in no case evolved hydrogen gas.

§ ii. *Examination of the gas exhaled by mushrooms placed under water.*

If mushrooms be placed in a receiver under water, at the end of a few minutes small bubbles of gas are seen to be disengaged. The following experiments were made upon different species of mushrooms exposed, 1st, to the direct rays of the sun; 2d, to perfect darkness; 3d, alternately to the sun and to darkness, during given intervals.

§ 1. *Mushrooms exposed to the sun.* Many species of mushrooms were exposed under water to the rays of the sun. At the end of some hours there was always developed some gas composed of hydrogen, of azote, and sometimes of two or three parts per cent. of atmospheric air. The quantity of the gases developed, and their proportion, varied according to the different species of mushrooms subjected to the experiment. The following are examples of it.

i. Three mushrooms of the species *agaricus leucocephalus*, placed under a receiver in the sun, disengaged, in six hours, two cubic inches of a gas composed of forty-two hydrogen and fifty-six of azote; the other two parts were of atmospheric air.

ii. Three stems of *sphæria digitata*, exposed in the same way, gave, at the end of ten hours, a gas containing sixty-five per cent. of hydrogen, and thirty-three of azote.

iii. Two mushrooms of the species *agaricus ericeus*, exposed during ten hours to the sun, disengaged one inch and three quarters of a gas containing fifty-five parts of hydrogen and forty-four of azote.

iv. Many individuals of the species *agaricus deliquescens*, exposed like the preceding, yielded, at the end of from six to eight hours, two cubic inches of a gas containing seventy per cent. of hydrogen, and the rest azote.

§ 2. In the case where the mushrooms were exposed to darkness, most frequently during twenty-four, and sometimes even during forty-eight hours, there was no disengagement of gas. Nevertheless, after a greater lapse of time, a little was generally produced; but it almost always contained less hydrogen, and more azote, than that which was disengaged from the mushrooms exposed to the sun. Thus, for example, two stems of the *agaricus contatus*, placed during more than twenty-four hours in a dark place, evolved no gas: at the end of sixty hours, only two cubic inches were evolved; while mushrooms of the same species, exposed to the direct rays of the sun, yielded the same quantity of gas at the end of six hours. The *sphæria digitata*, placed under the same circumstances, afforded results altogether analogous.

§ 3. Many individuals of the species *agaricus physalloide*, were placed in water during forty-eight hours, in perfect darkness. At the end of this time, there was found only one bubble of gas in the receiver; the mushrooms remained perfectly fresh, inodorous, and shewed no tendency to decay. Having then exposed them to the sun, at the end of two hours I observed two cubic inches of a gas composed of fifty-one parts hydrogen and forty-three of azote. The *bolĕtus aurantiacus*, the *agaricus campestris*, and many other species, presented the very same phenomena.

§ 4. *Cause of the phenomenon.* The disengagement of hydrogen which takes place, when recently gathered mushrooms are placed in water, appears to me a fact worthy of remark. Does this phenomenon arise from a species of vegetation which continues to take place in the water, and by which this, being decomposed, would yield its oxygen to the mushroom, and allow its hydrogen to be disengaged, or is it the result of an incipient putrefaction? The following reasons induce me to believe that the disengagement of hydrogen gas arises from the first of these two causes, that is, from a sort of continuance of vegetation, rather than from the second.

i. The mushrooms I subjected to experiment were always perfectly fresh; the moment they shewed any sign of putrefaction, I discontinued the experiment.

ii. Some extremely coriaceous mushrooms, in which putrefaction could not shew itself till after a very considerable time, such as the *sphæria digitata*, have evolved in many cases, and, at the end of a short time, a considerable quantity of hydrogen gas. On the other hand, mushrooms of a much softer consistence, and more disposed to putrefaction, frequently disengage only a small quantity of this gas. It is thus that M. de Candolle has proved that the *peziza nigra*, which belongs to the last case, disengages only fourteen per cent. of hydrogen, while the *sphæria digitata*, a very coriaceous mushroom, according to the same botanist, and as appears from his own experiments, disengages from sixty-five to seventy per cent. of this gas.

iii. At an equal temperature, the disengagement of gas appears to take place more rapidly in the sun than in darkness. Nevertheless, light has never been assigned as a cause favourable to putrefaction, while, on the contrary, it has a great effect upon vegetation.

iv. Lastly, in admitting that the disengagement of gas is the result of this species of vegetation of the mushroom, the presence of a great quantity of azote is easily explained by supposing that it arises from the decomposition, either of the air which is included in the water, or of that which exists in the pores, and in the very tissue of the vegetable.—*Annales de Chimie*, xl. 318.

17. *Extraordinary effect of an Earthquake at Lima*, 1828. (Communicated by Captain Bagnold.)—Dear Sir,—Having experienced, during my residence at Coquimbo on the coast of Chili, no less than sixty-one smart shocks of earthquake in twelve months, without taking minor ones into consideration, I was induced to obtain, from an officer of H.M.S. Volage, the particulars of the destructive visitation which occurred at Lima in 1828. As one of the effects produced appears to me worthy of record, I transmit it you for a place in the Journal.

On the 30th of March, H.M.S. Volage was lying moored with two chain cables in the bay of Callao; the weather was remarkably fine and clear, when, at half-past seven o'clock, a light cloud passed over the ship—at which moment the noise usually attendant on earthquakes in that country, resembling heavy distant thunder, was heard; the ship was violently agitated; and, to use the words of my informant, "*felt* as *if placed on trucks, and dragged rapidly* over a pavement of *loose stones*." The water around "hissed as if hot iron was immersed in it;" immense quantities of air-bubbles rose to the surface, the gas from which was offensive, resembling, to use my friend's phraseology again, "rotten pond mud;" numbers of fish came up dead alongside; the sea, before calm and clear, was now strongly agitated and turbid; and the ship rolled about two streaks, say fourteen inches, each way. A cry of "there goes the town," called my friend's attention towards it: a cloud of dust, raised by the agitation of the earth and the fall of the houses, covered the town from view, whilst the tower of the garrison chapel, the only object visible above the dust, rocked for a few seconds, and then fell through the roof; and, from the high perpendicular rock at the north end of the island of St. Lorenzo, a slab, supposed thirty feet thick, separated from the top to the bottom of the cliff, and fell with a tremendous noise into the sea. The wharf, or pier, was cracked three parts across, shewing a chasm of eighteen inches wide ; the chronometers on shore, except those in the pocket, and most of the clocks, stopped, whilst the rates of chronometers afloat were, in many instances, altered. A great number of lives were lost ; amongst whom were four priests killed in the churches, one of them by the falling of an image, at whose base he was at prayer.

The Volage's chain cables were lying on a soft muddy bottom in thirty-six feet water ; and, on heaving up the best bower anchor to examine it, the cable thereof was found to have been strongly acted on, at thirteen fathoms from the anchor and twenty-five from the ship. On washing the mud from it, the links, which are made of the best cylinder wrought iron, about two inches in diameter, appeared to have undergone *partial fusion* for a considerable extent. The metal seemed run out in grooves of three or four inches long and three-eighths of an inch diameter, and had formed (in some cases, at the ends of these grooves and, in others, at the middle of them) small spherical lumps or nodules, which, upon scrubbing the cable to cleanse it, fell on the deck. The other cable was not injured, nor did my friend hear of any similar occurrence amongst the numerous vessels then lying in the bay. The part of the chain so injured was condemned, on the vessel's being paid off at Portsmouth, and is now in the sail-field of the dockyard ; and I should think a link of it would be worth preserving in the museums of the different scientific bodies.

That the phenomena of earthquakes are produced by volcanic explosion, there can be little doubt ; and that they are frequently accompanied by powerful electric action has long been known : to which of these causes are we to look for the powerful effects here witnessed ? I remain yours faithfully,

THOS. BAGNOLD.

Knightsbridge, June 14, 1829.

18. *Substance which fell from the Atmosphere in the Province of Romoe, Persia.*—The packet containing this substance was sent to France, and confided to the hands of M. Thenard : it was labelled, " Grains which fell from the sky in Persia ;" and accompanied by a note, which said, that " Lieut.-Gen. Count de Soklen, who served in Persia, has lately received from that country a small quantity of grains, which fell from the sky, and which, in some places, covered the earth to the depth of six inches. This astonishing rain fell in the province of Romoe, not far from Mount Ararat. It appears not to be the first time ; for the inhabitants say they saw it happen in 1824. This unexpected substance was gathered in the beginning of April ; but unfortunately, the direction of the wind at the time was not noticed. Sheep first ate of the substance ; after which, men gathered it, and made a very passable sort of bread.

MM. Thenard and Desfontaines soon ascertained that it was probably a lichen, and perhaps belonging to the genus *Lecidea.* Chemical analyses also indicated the substance to be a species of lichen. M. Thenard remarks, that it is easy to suppose how lichens may form layers six inches in thickness in particular places; considering, that when transported from their original locality by the winds, they ought to accumulate here and there, according to the nature of the ground, and the obstacles naturally offered in different situations.—*Ann. de Chimie*, xxxix. 422.

19. *Earthy Powder which fell at Idria, in Carniola.*—On the 14th of March, 1813, being at the mines of Idria, whilst much snow was falling, M. Legallois observed that the surrounding mountains appeared of a reddish tint. Calling the attention of others to the appearance, it was soon ascertained that snow of the particular colour was falling. It continued several hours, and then resumed its usual white state. Several days after, when the snow had sunk together, and was about eighteen inches thick, on making a section of it, a band, two inches thick, was observed, of a brick-red colour, more sensible than it was remarked to be at the time of falling. Some of this red snow was collected, melted, and the subsidence from it sent to M. Vauquelin for examination.

M. Vauquelin describes the substance as being of a yellow red colour, exceedingly divided, yet producing a sound between the teeth, and containing a few white particles resembling mica. Being analysed, it gave,

Silica	36.75
Alumina	11.75
Carbonate of lime	17.50
Iron	6.25
Titanium	3.75
Organic matter	24.00
	100.00

The extreme division of the matter assists the mind in imagining how it could be raised into, and carried along by, the air; but it must be supposed to exist at a greater or smaller distance in a pulverulent state; and from the fineness of the matter, there is reason to think it must have come from a great distance. It may be supposed, too, that finely divided vegetable matter must have existed at the same place; but this may have come from other localities; and, in fact, the first snow which fell being dissolved, gave a coloured solution, and traces of organic matter. The most remarkable circumstance is, the presence of such a quantity of titanium, a metal which is rather rare at the surface of the earth.—*Ann. de Chimie*, xxxix. 438.

20. *On a new Mineral Hydro-carbon.*—A combustible substance has been found by Colonel Scherer in the interstices of lignite or fossil wood, discovered near Urnach, in the canton of St. Gall;

and being given to M. Macaire Prinsep, he has examined, and published an account of it. It occurs in small white, or yellowish white, acicular crystals, between the fibres of the fossil wood, or else in translucent layers, having a nacreous lustre, no taste or smell, and a specific gravity of about 0.65 *. Heated, it fuses, and remains liquid when cooled, until touched, when it suddenly crystallizes. It melts at 112° F., distils at 194° F., and condenses unaltered. It sinks in alcohol at 40°, (s. g. .817 ?) dissolving only slowly, unless heat be applied. It dissolves in ether and oil of turpentine, not in water or alkaline solutions. It differs from naphthaline in being more fusible, more volatile; in the form of its distilled product, which, with naphthaline, is frequently that of rhomboidal plates; in its inferior solubility in alcohol, and also as it appears in composition.

M. Macaire Prinsep analysed both naphthaline and the substance, by the use of peroxide of copper; but appears to have doubts, not of the principle of the process, but of his results. However, he makes out naphthaline to consist nearly of 86 carbon, and 13.8 hydrogen; this is almost equal to single proportionals of each, or 6 and 1 by weight, which is the composition of olefiant gas; but both Ure and Thomson make out far less of hydrogen.

The new fossil matter is, according to M. Macaire Prinsep, composed of 73 carbon, and 24 hydrogen, or nearly of one proportional of carbon to two of hydrogen; 6 : 2 by weight. This is the same composition as the light hydro-carbon.—*Bull. Univ.* xl. 68.

If the latter estimate be at all near the truth, then it would be exceedingly interesting to know the specific gravity of its vapour, for comparison with light hydro-carbon; and also the kind of flame which is produced when the vapour of the substance is burnt from a jet. No indication of the appearance of the flame, whether pale, bright, or smoky, is given.—Ed.

21. *Fall of Rain at different heights.*—The fall of rain at the Observatory of Paris, in the year 1828, was 23.04 inches on the terrace, and 24.71 inches in the court-yard, the former being 91.86 feet above the latter.

22. *Dimensions of the Six principal Lakes in the United States.*—

Lakes.	Length.	Width.	Depth.
Ontario	180 miles	40 miles	500 brasses.
Erie	270	60	200
Huron	250	100	900
Michegan	400	50	unknown
Green Bay	105	20	unknown
Superior	480	109	900

Phare du Havre.

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* There must be some mistake here, for afterwards it is said to sink in alcohol.—Ed.

23. *Hints on the Antiquity of Languages, and on the Origin of Alphabetic Writing.*—It is natural to suppose that the primeval languages of mankind must have been polished and refined in the long period of more than two thousand years which elapsed between the creation and the flood. The antediluvians were acquainted with music, and proficients in various sciences. Playfair has shewn, in the second volume of the Edinburgh Transactions, that astronomical observations commenced more than one thousand years before the deluge. This accords with Josephus, Antiq. i. 2; and, therefore, unless we reject the testimony which supports these facts, we must believe that words corresponding to these several degrees of knowledge, as they were acquired, must then have existed. Consequently, if the original language of mankind were preserved by Noah, and, from the absence of any of his descendants from the plains of Shinar, escaped from the confusion of tongues, among all the languages of antiquity it must necessarily be that one which appeared in its maturity while all others were in their infancy, which allowed the greatest precision and accuracy in discriminating between the relations of objects, whose nervous and musical structure was best adapted to the force of eloquence and the harmony of verse. This, surely, is not the Hebrew. It may be alleged, as it has been by Dr. Louth, de Sac. Poes. Hebræ., that from our limited acquaintance with this tongue we are incapable of deciding on its merits; but among a people so strictly national as the Jews, if any noble monuments of literature, besides those of which we are possessed, had ever existed, some more would have floated down the stream of time, and some other names would have been recorded than those of the lost chronicles of Israel and the book of Jasher.

But while every other language is more or less defective, the Greek is of an opposite character. Cadmus, arriving from Phœnicia, introduced letters into Greece about 1543 B. C. Scarcely fifty years afterwards, the poet Linus is supposed to have flourished, and the celebrated Orpheus, almost his contemporary. Nearly at that period many other poets are mentioned by Eusebius, Præp. Evan. x. 2. Now in what manner to account for this apparent precocity, if such a term may be employed, the immeasurable disproportion between the state of civilization of those who spoke this language, and who, from their ceaseless intestine wars, had no leisure to attend to its improvement, and the degree of perfection the language itself had attained, is a transcendent problem, of which the only solution seems to be, that although, from the want of alphabetic characters, there could be no writers to detail its early history, yet the Greek tongue had been gradually refined and augmented during the lapse of more than two thousand years. Our limits preclude us from entering into all the proofs by which this opinion can be maintained; but the name of this language, *Ionic* or *Hellenic*, evidently points out that it descended directly from Noah, in the line of Japhet, the grandfathe of *Elisha*, or, as it is written in the Samaritan copy, *Helas*, the son of *Javan* or *Fou*. According to Strabo, (viii.) the ancient inhabitant of Attica were Ionians; and Herodotus, i. 58, says that in his opinion these people always spoke the same language.

The Phœnician, the Arabic, the Canaanitish, and the Hebrew have always been considered as dialects of the same tongue, Vitringa, lib. i., and Lowth, Prælect. 32; and according to Mr. Astle, the Phœnician and Egyptian languages were very similar; and as the Hebrew was not, according to M. Lacour's hypothesis, Essai sur les Hiéroglyphes, in Egypt, as the Zend was in Persia, a sacred language, so neither is there any reason for supposing that it was a superior or a court language, as the Deri to the other dialects of the Persian tongue. The opinion, however, that the Hebrew and Egyptian tongues were originally the same, has been long since advanced by the learned Vitringa. In lib. i., cap. 3, of his Origines Sacræ, he expressly says, "cum unâ eâdemque linguâ omnes orientis populos diu post extructionem civitatis Babyloniæ usos esse probavimus, haud obscure indicavimus, credere nos, illam linguam (scilicet Ægyptiacam) fuisse vel Hebræam, vel Hebrææ simillimam."

Mr. Bryant has, we may almost say satisfactorily, proved that the ancient mysteries were designed to commemorate the flood, its causes, and catastrophe. If, as he supposes, all these ceremonies originated in Egypt, there would be presumptive evidence that Noah died there: for who would be so likely to establish this commemoration as the person to whose single family such mercy was granted? and, therefore, whatever antediluvian records were preserved in the ark, it might be expected would be met with in Egypt. Moses speaks of "the book of the generations of Adam." The Sabæans had a book written by Adam: "the book of Enoch"* is mentioned, and the Ambres: these last may be apocryphal; the originals, like the Zendavesta, may have been lost, and others composed from a recollection of their contents, Hyde de Nat. Vet. Pers. 568; but there must have been some grounds for believing their existence. And as from the pastoral lives of the patriarchs, it is not probable they would have any but oral traditions; and as Moses was "learned in all the wisdom of the Egyptians," we may conclude that from these books he compiled his history, inserting in their proper places the fragments of metrical legends, Genes. iv. 23, 24. xv. 13. Lowth, Præl. 4. which, from the interesting events connected with their origin, had been the longest remembered. If, as appears from the researches made latterly in Asia, and still more recently in America, mysteries similar or analogous to the Egyptian ones were celebrated there; and if, from any light which oriental literature may hereafter throw upon the subject, it should be discovered that any books which relate to antediluvian occurrences have existed or still exist in the east or the west, we may, perhaps, conclude that these "books of the generations," or whatever they were, were common to the three sons of Noah; and that those solemn rites to perpetuate the memory of their miraculous escape were instituted by all the individuals who shared in it, and that they were gratefully performed by all their descendants who preferred a settled to a wandering life.

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* Page 2. From internal evidence in the work itself, "the book of Enoch seems to have been composed about 30 B.C." See the learned preface of the editor.

But there are no grounds for the opinion that either the books mentioned above as antediluvian, or the literary monuments in Greece which were lost in a flood, (Diod. Sic. v. 74.) probably the same as the former ones, were written in alphabetic characters. From the extreme improbability that whosoever had enjoyed the inestimable advantage of alphabetic, would not have abandoned them for hieroglyphic characters, we conclude that the Chinese and Egyptians never possessed the former; and as, if one of the children of Noah had been acquainted with them, all would have been so, and as two of the most ancient nations were ignorant of them, we hesitate to believe that the Sanscrit alphabet is coeval with the flood. (Sir W. Jones, Trans. Asiat. Soc.) Mr. Astle has endeavoured to shew, from the Books of Moses, that alphabetic writing is spoken of before the Tables of the Law were given, alleging that as Moses was commanded to engrave on stones and on plates of metal, he *must* have known the use of alphabetic letters, to which we cannot assent, (the cuneiform characters, the lapidary writings of the Persians, lately deciphered by Mr. Price, "Antiquities of Persepolis," seems to be a case in point,) and attributes to the Phœnicians the merit of the discovery. This is contrary to the direct evidence of Diodorus, v. 74, who says that the Phœnicians only changed the form of the letters, but did not invent them. From there being no direct testimony to prove the existence of letters prior to the time of Moses, but, on the other hand, from no writing being specified on an occasion where it would seem to be required, (Genesis xxxi. 46.) and from alphabetic characters having been used shortly after the exodus; from the voice of antiquity, which describes letters as a gift of the gods to men; from the casual mention of Moses as the inventor of the Hebrew letters; and from all of them, with the exception of Teth, being comprised in the Decalogue; from the Coptic, Samaritan, Greek, Hebrew, and Syriac alphabets manifestly having but one root, it seems very probable that alphabetic characters date from the æra when the Law was given from Sinai.

With all due allowance for the knowledge he possessed from tradition and revelation, there is abundant internal evidence that Moses composed his *history* from the Egyptian books mentioned above; but that these books were very little beside a bare catalogue of significant names, is evident from the dry, sterile "book of the generations of Adam," compared with the more noble style that distinguishes the rest of his work. Some of the ablest commentators on Scripture (Grotius and Le Clerc) have thought that the names recorded in the Pentateuch have been translated into the Hebrew,—the persons themselves being denoted by hieroglyphic figures, representing some action or circumstance by which they could be identified,—at the time a sort of universal graphic language, comprehended by all mankind, but absolutely spoken by none, as the Chinese written language is intelligible when the oral dialects of it are not understood.

24. *Comparison of the Alps, the Pyrenees, and the Mountains of Scandinavia.*—The following comparison between the three great chains of European mountains, the Alps, the Pyrenees, and the mountains of Scandinavia, has been made by Professor Schouw, of Copenhagen. The Pyrenees lie between 42 and 43½° N. lat.; the Alps between 43½ and 48°; the mountains of Scandinavia between 58 and 71°. The Alps and Pyrenees are consequently at an almost equal distance between the Equator and the North Pole; the Scandinivian chain extends to the polar circle; the Pyrenees are not more than a degree of latitude in breadth; the Alps occupy four degrees and a half; the Scandinavian mountains thirteen. It is clear that these last must present a greater variety of temperature, and have, in general, a much colder climate. In longitude the Pyrenees extend from 16 to 21°, the Alps from 22½ to 35°, and the Scandinavian mountains from 22½ to 48° from the meridian of Ferro. The heights are well known of the Alps and Pyrenees, but those of the more remarkable of the Scandinavian chain, never having before appeared in print, we insert at length.

	feet		feet
Sulitelma	5800	Soendre Skagestarlting	7600
Areskutan	4500	Koldeting	6800
Syltop	5500	Mugnafield . . .	6800
Tronfield	5300	Justedalsbræer . . .	5500
Sneehætten . . .	7100	Subetind	5500
Pikhætten	6400	Hallingjoekel . . .	5400
Lodalskaabe . . .	6200	Hartrig	5200
Lomseg	6200	Folgefond	5000
Nordre Skagestarlting	7100	Gousta	5800

25. *Meteorological Observations made at Geneva and at Saint Bernard.*—The observations at Geneva for these last thirty-two years have been in different places, and at different times of the day: calculated for the same situation, they give for the mean temperature, 7·85 of Reaumur; that of St. Bernard, from ten years' observation, is 0·79 of Reaumur. The quantity of water which has fallen at St. Bernard in the shape of rain, snow, or hail, is to the quantity which has fallen at Geneva in the same time : : 7 : 3; a very curious result. The mean barometric pressure at Geneva is 26 inches 10 lines; at St. Bernard 20 inches 9·75 lines.—*Bibliot. Univ. Mars.*

26. *Coincidence of Storms and Earthquakes with a Depression of the Barometer.*—February 21, 1828, at three P.M. the barometer at Geneva indicated 26 inches $\frac{11}{16}$ of a line. The 19th, 20th, 21st, and 23d of the same month, furious tempests raged throughout the south of Europe, and on the 23d the shock of an earthquake

was felt in the north of France and in the Netherlands; a new example of the coincidence of these three phenomena.—*Bibliot. Univ. Mars.*

27. *Account of some remarkable Mongrels between a Dog and a she Wolf*, by M. Wirgmann, jun. 'Isis,' Nos. 8 and 9.—The following occurrence is very similar to what took place two years since at the menagerie of the Island of Peacocks, near Potsdam. A pointer dog impregnated a she-wolf, and three female whelps, very different from each other, were brought into the world; one of them died; one bore a stronger resemblance than the other two to a wolf in the quality of its hair, having on its leg the black line which characterizes this animal; the second also was sufficiently like the mother, with the exception of the eyes, which were the same as those of the dog; the third pup was very nearly a pointer, although in some respects incomplete; its character was gentle, while the other had a fierce air. The father was quite white, with brown spots; on the other hand, the young ones had, the first, the hide and colour of a wolf; the second was whitish on the mouth, on the cheeks, on the sides of the neck, and the back blackish; lastly, the third, a white band from the front of the neck, over the breast and the back, the same colour as that of the second.

28. *Urinary Calculi.* Naturwissensch. Abhand. v. e. Gesellsch. in Wurt.—M. Rapp, having analysed eighty-one urinary calculi which he had obtained in different parts of the kingdom of Wurtemberg, has obtained the following results:—

1 Calculi, with oxalate of lime (almost always with a nucleus of uric acid)	
a) Calculi, formed by oxalate of lime alone	22
b) Oxalate of lime, with an exterior fusible coat (of ammoniaco-magnesian phosphate)	28
c) Oxalate of lime, covered with a thick coat of uric acid	3
d) Oxalate of lime mixed with a considerable quantity of uric acid, so that their external characters are no longer distinguishable	3
2 Uric acid	7
3 Uric acid, covered with one or more fusible coats	9
4 Urate of ammonia	1
5 Fusible calculi without a distinct nucleus	7
6 Phosphate, with a considerable proportion (0.13) of carbonate of lime	1
	81

One remarkable fact is the rarity in Wurtemberg of calculi formed of uric acid, if the number of them be compared with that which is generally found in England. M. Rapp has found 7 out of 81 in his collection; while in the collection of Guy's Hospital in London, 22 are met with among 87; at Norwich 66 out of 181 (Marcet); at Manchester 71 of 187 (Henry), without counting 39 others composed of uric acid and of phosphate; of 150 calculi in Hunter's museum, and in the collection of Sir E. Home, there are 61 formed of uric acid, including 45 containing a small proportion of phosphate (Brande). In the collection at Bristol, Smith found 73 calculi of uric acid out of 218, the total number.

Calculi of uric acid are not, therefore, common in Wurtemberg; this substance, however, is the most generally diffused throughout them. M. Rapp counted 57 of which only the nucleus was formed of it. This nucleus is frequently very small, sometimes only a line in diameter; in many of these concretions the oxalate of lime is mixed with uric acid; sometimes but rarely, coats of uric acid alternate with coats of phosphate.

M. Rapp found no stone composed entirely of phosphate of lime, but frequently this salt was mixed with ammoniaco-magnesian phosphate. The phosphated earths form but rarely the first nucleus of a stone; most frequently they form the external coats when the nucleus is already formed. This is frequently observed in mulberry calculi; occasionally a certain quantity of carbonate of lime is mixed with the phosphates. These calculi may be formed without any particular diathesis, and in a man in good health, whenever any extraneous body gets into the bladder, they become the most voluminous calculi. Earle described one (Philos. Trans. 1807) that weighed four ounces. An animal regimen appears singularly favourable to the deposition of phosphated earths in the urine.

The presence of urate of ammonia in urinary calculi has been contested by MM. Brande, Henry, and Marcet; and the signs which Prout has indicated to recognize this salt are all doubtful. M. Rapp has, however, found it in three calculi, two of which had a crust of oxalate of lime, and the third a crust of ammoniaco-magnesian phosphate. It is best recognized by means of a solution of caustic potash, in which these calculi completely dissolve, disengaging ammoniacal gas; but it is necessary to be certain that they do not contain ammoniaco-magnesian phosphate. Silica has not been found in any of the calculi examined by M. Rapp. Calculi of oxalate of lime are the most common in the kingdom of Wurtemberg; in England, on the contrary, they are rare; in the former there were 56 out of 81, while at Norwich only 59 out of 181; in the Hunterian Museum 6 out of 150; 28 out of 87 at Guy's Hospital; 11 out of 187 at Manchester, and 65 out of 208 in the collection at Bristol.

29. *On the Animal of the Silicaria.*—M. Audouin has announced to the Philomathic Society of Paris that he has observed the animal of the Silicaria, of which only the calcareous tube has as yet been known. Zoologists were uncertain of the class to which this animal belonged, some placing it among the annelides, and the others among the molluscæ. M. Audouin announces that by its organisation it belongs to the class of molluscæ, and approximates to the genus *vermet* of Adanson: it is provided with a very thick and cor-

neous operculum. Its coriaceous skin, denominated by the French "manteau," is divided from one extremity to the other, and, contrary to the assertion of M. de Blainville, the branchiæ are found on one side only, the left. The body is terminated behind by a "tortillon," the head, which is distinct, is provided with two eyes situated at the base of two cylindrical tentacula, slightly swelling at the extremity.—*Annales des Sciences Nat.*

30. *Observations on the influence of Cold on New-born Children.*—Dr. Trevisan has been making researches in Italy, principally at Castel-Franco, analogous to those of MM. Villermé and Milne Edwards in France. The conclusions at which he arrives, are:—i. In Italy, of 100 infants born in December, January, and February, 66 died in the first month, 15 in the course of the year, and 19 survived;—ii. Of 100 born in spring, 48 survive the first year;—iii. Of 100 born in summer, 83 survive the first year;—iv. Of 100 born in autumn, 58 survive the first twelve months. He attributes this mortality of the infants solely to the practice of exposing them to cold air a few days after their birth for the purpose of having them baptised at the church. As well as MM. Milne Edwards and Villermé, Dr. Trevisan calls the attention of the ecclesiastical authority to measures suited to put a stop to such disasters without violating the precepts or practices of religion.

31. *Elevation of the Principal Table-lands of the Globe.*—Of the different table-lands of which the elevation has as yet been determined, the following is a correct estimate in English feet. The mean height of the department of La Haute Vienne, in France, is 1847·14. The most elevated table-land of the centre of France is that of Auvergne, which serves as base to the mountains of the Mont-d'Or, Puy-de-Dôme, and Cantal; its mean height is 2362. In Spain, the soil of the two Castiles is 1903; in Switzerland, the plains of the canton of Berne are only 1312, or 1369, although the mountains in their vicinity are 12,796 feet high, and one of them, Mont Blanc, 15,666. These plains have nearly the same elevation as the table-land of Suabia, Bavaria, and New Silesia. In Africa, all the table-land to the north of 31°, inhabited by the Betjuans and other people, rises 5807 above the sea. In the New World, the surface of the great Mexican plateau, which is inhabited, is from 5570 to 8858. (The pass of the great St. Bernard is only 8173.) These elevated regions, which are much colder than the lower lands in their neighbourhood, have the same temperature as France. To give a general idea of the effects produced on the temperature by difference of height, we may add, that from the observations of M. de Humboldt in America, and of Saussure in the Alps, an elevation of 623 feet corresponds to a diminution of one degree of the centigrade scale.

32. *Mortality among Leeches during Storms.*—That atmospheric changes have a remarkable influence upon leeches, is a well established fact. In 1825, M. Derheims of St. Omer, ascribes the almost sudden death of them at the approach of, or during storms, to the coagulation of the blood of these creatures, caused by the impression of the atmospheric electricity. This opinion, which at that time was the result of theory, he confirmed, in the month of March last, by direct experiment.—*Ferussac's Bul.*

33. *Pinite found in the Granite in the Environs of Heidelberg,* by M. Blum, 'Zeitsch. für Mineralogie.'—The author gives a very detailed description of the pinite found in the granite near Heidelberg, comparing it to that of Auvergne and of Saxony, and dwells much on the different varieties of this mineral found in many different localities.

At the end of this memoir is a translation of an unpublished article of a Swedish journal, on a new species of mineral, under the name of Pyrargillite. This substance was found at Helsingfors, in Finland. It contains 15.8 per cent. of water, of an odour and savour that was bitter, without a trace of acid, much alumine, a little silex and magnesia, and a trace of phosphoric acid. It differs from Cordierite by being less hard, in that respect being intermediate between calcareous and fluor spar; from Fahlunite, by the same character, and its specific gravity of 2.505; from pinite, by this last character, and by its splitting with heat.

34. *Description of the Spiricella,* a new genus of fossil shells. By M. Rang, 'Bulletin d'Hist. Nat. de Bordeaux.'—This fossil, of which M. Rang thinks right to make a new genus, under the name of Spiricella, belongs to the tertiary formation of Bordeaux. Its characters are: shell much flattened, elongated, arched, with sharp margins; summit spiral, heterostrophe constructed horizontally, situated behind and to the left, open on its inferior face; an indistinct impression, but occupying particularly the posterior half of the shell, where it appears nearly parallel to the margin.

Observtion.—The animal was certainly much larger than its shell.

35. *Belemnites.*—M. Raspail, an eminent French naturalist, after an accurate investigation of 250 belemnites from the mountains of Provence, has discovered that belemnites are not, as geologists usually suppose, the shells of animals, but cutaneous appendages of a marine animal, perhaps allied to the echinodermata, but which are now extinct. In the number for February of the Annales des Sciences d'Observation, a journal edited by M. Raspail, that gentleman has given the following synoptical table of his classification of these interesting objects:—

Tabella Synoptica Belemnitarum Alpium.

- Ferruginei
  - lati
    - petalopsides
      - Bel. Emericii.
      - — pileus.
      - — binerrius
      - — distans.
      - — acinaciformis.
      - — elegans.
      - — truncatus.
      - — linearis.
  - saltem basi teretes
    - obesi
      - — obesus.
      - — gracilis.
      - — acutus.
      - — extinctorius.
      - — asulcus.
    - integri
      - — integer.
- Cornei.
  - lati
    - dilatati
      - latere angustiore caniculato
        - — variegatus.
        - — formosus.
        - — apiculatus.
        - — convexus.
      - latere non caniculato
        - apice integro
          - apice obtuso
            - — sinuatus.
            - — spathulatus.
            - — ellipsoides.
            - — complanatus.
          - apice acuto
            - — pisciformis.
            - — delphinus.
            - — bifurcatus.
            - — angustus.
            - — triqueter.
            - — pseudoformosus.
        - apice lobato.
          - apice sursum lobato
            - — emarginatus.
            - — amorphus.
            - — difformis.
          - apice retro lobato
            - — mitra.
            - — mitraformis.
            - — persona tonsoria.
    - vermiculati
      - — honorati.
  - saltem basi teretes
    - sordidi
      - — rimosus.
      - — depressus.
      - — incurvatus.
      - — marginatus.
      - — attenuatus.
      - — pistilloides.
    - Pelorii
      - — gibbosus.
      - — gemmatus.
      - — rostratus.
      - — navicula.
      - — brevirostris.
      - — fusus.
      - — oblongus.
      - — rugosus.
    - claviformes
      - sulcati
        - basi nonattenuatâ.
          - — minaret.
        - basi attenuatâ
          - — hastatus.
          - — symmetricus.
          - — subfusiformis.
          - — præmorsus.
          - — contortus.
      - asulci
        - — pistilliformis.
        - — crassia.
        - — crassissimus.
        - — aculeus echini.
- bisulci
  - — bicaniculatus.
- polygoni
  - — polygonalis.

36. *Temperature of the Sea at different Depths.*—M. Lenz, the natural philosopher who accompanied the last Russian expedition round the world, made a series of observations on the temperature of the ocean at different depths, which have not as yet been laid before the public. The following are the results:—

Lat. N.	Long. from Greenwich.	Depth in Fathoms.	Temp. by Cent. Scale.
° ′	° ′		
7 20	21 59	0	25.80
——	——	539	2.20
21 16	196 1	0	26.40
——	——	140.7	16.36
——	——	413.0	3.18
——	——	665.1	2.92
——	——	914.9	2.44
25 6	156 58	0	21.50
——	——	167.0	14.60
32 6	136 45	0	21.45
——	——	89.8	13.35
——	——	214.0	6.51
——	——	450.2	3.75
——	——	592.6	2.21
32 21	42 30	0	20.86
——	——	1014.8	2.24
41 12	141 58	0	15.20
——	——	205.0	5.16
——	——	512.1	2.14
45 35	15 17	0	14.64
——	——	197.7	10.36
——	——	396.4	9.96

We may remark that these observations, executed with every precaution necessary to ensure extreme accuracy, were made at greater depths than any hitherto attempted, Irvine having reached only 683 fathoms, and Péron 351.

37. *Vegetating Fungus found in the Stomach of a Cod.* Cabilian Strom. Morhua, Lin.—A French naturalist relates, that a fisherman brought him three pebbles, about the size of the first joint of a large thumb, on which were implanted by adhesion (empatement) plants and rudiments of plants, of a fucus kind, which was identified as the fucus confervoides, described by Bertolini, in his ‘Amœnitates Italiæ.’ On one of the three stones was found an unique plant of considerable size, and nearly two feet in length, in active vegetation. Its colour was a deep bottle-green, except in one part, which formed the ramified summit, and which extended by the “arriere bouche” of the animal; this part, nearly two inches long, was transparent, of a pale violet-red, brittle, and more swollen than the lower

branches, which are green, flexible, and sufficiently tenacious; above a second stone, a plant two-thirds shorter than that on the first stone, was growing. To this was attached a plant about three inches long, to the two sides of which, and at from one and a half to two lines distance, two adhesions, not much smaller than that of the principal plant, were visible, and from which issued, in the shape of points bent back into hooks, and two lines in length, the rudiments apparently of two new plants. Another adhesion placed laterally, and of less extent, bore, as the germ of a third plant, a straight point, one line and a quarter in length. Opposed to the plant which was developed, and in the direction of the length of the stone, was the germ of a fourth plant, two lines long, and also bent into a hook. The two other stones had no similar germs of new plants, but they might have been detached without leaving any traces behind; drying produced the spontaneous separation of the others, and the plant itself then came off with the least touch; the place it had occupied could not then be discovered. The method of attachment resembled an adherence, by excluding the air. One of the stones was of gneiss with amphibole, another of gneiss only, the third of a sort of quartz. One of them was found in the curvature of the stomach of a cod, the two others in the large diameter of it. All adhered strongly to the substance of the stomach, and were obliged to be cut out. This fucus then can grow and spring from its seed, whatever that may be, in the stomach of a cod-fish; also, its force of vegetation prevails over the digestive force of the animal, unless it be that the fish, being entirely carnivorous, does not digest herbs. In one only, according to the testimony of the whole body of our fishermen, a piece of wood, the size of a man's fist, was found imbedded in the substance of the stomach of a cod.

METEOROLOGICAL DIARY for the Months of March, April, and May, 1829, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

FOR MARCH, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Sunday	1	19	37	30.08	30.02	E	ENE
Monday....	2	29	35½	30.00	30.06	NE	NE
Tuesday ...	3	32	37	30,14	30.14	E	N
Wednesday.	4	32	43	30.08	30.07	N	ENE
Thursday ..	5	28	43	30.07	30.04	ENE	NE
Friday	6	36	46	29.98	29.90	NE	NE
Saturday...	7	36	46½	29.85	29.84	NE	NE
Sunday	8	37	47	29.84	29.83	NW	N
Monday....	9	36	49	29.79	29.70	W	NW
Tuesday ...	10	32	46	29.70	29.70	NE	NE
Wednesday.	11	25	43	29.70	29.70	NE	E
Thursday ..	12	29	40	29.64	29.60	E	EbN
Friday.....	13	32	42	29.53	29.57	EbN	NE
Saturday...	14	29	42	29.68	29.68	W	NW
Sunday	15	21	40	29.72	29.74	E	NE
Monday....	16	17	40	29.70	29.51	NE	ESE
Tuesday ...	17	16	46	29.40	29.30	E	W
Wednesday.	18	20	51	29.50	29.49	SbE	S
Thursday ..	19	43	59	29.48	29.37	SSE	SE
Friday	20	45	58	29.30	29.55	SW	SE
Saturday...	21	31½	53	29.90	29.90	SE	E
Sunday	22	28	53	29.80	29.63	ESE	E
Monday ...	23	34	49	29.68	29.70	ENE	NE
Tuesday ...	24	33	44½	29.79	29.72	NE	NE
Wednesday.	25	20	44	29.76	29.80	NE	NE
Thursday ..	26	30	45	29.80	29.80	SW	SE
Friday.....	27	33	49	29.80	29.70	SE	E
Saturday...	28	25	53	29.60	29.40	ENE	NE
Sunday	29	36	46	29.23	29.20	NE	NE
Monday ...	30	37	46	29.17	29.12	NE	NE
Tuesday ...	31	36	45	29.12	29.12	E	NE

FOR APRIL, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Wednesday.	1	30	45	29.21	29.27	NW	NW
Thursday ..	2	24	46	29.30	29.33	W	WNW
Friday	3	27	47	29.45	29.50	W	W
Saturday...	4	30	49	29.50	29.40	WSW	SE
Sunday	5	40	54	29.22	29.10	SW	SW
Monday....	6	39	54	29.00	29.00	SW	SW
Tuesday ...	7	30	51	29.00	29.03	SW	W
Wednesday.	8	30	53	29.20	29.20	SW	S
Thursday ..	9	33	44	29.20	29.07	E	SW
Friday.....	10	34	53	29.18	29.27	SW	WSW
Saturday...	11	34	53	29.40	29.38	SW	SE
Sunday	12	43	59	29.07	29.01	SW	SE
Monday....	13	45	54	28.90	28.96	S	SbE
Tuesday ...	14	32	57	29.08	28.90	SW	SE
Wednesday.	15	37	54	28.78	29.05	SE	SW
Thursday ..	16	40	48	29.10	29.17	SE	WbS
Friday.....	17	31	54	29.44	29.51	SW	SW
Saturday...	18	37	57	29.58	29.63	WSW	WbS
Sunday	19	36	54	29.50	29.48	WbS	W
Monday....	20	38	56	29.52	29.68	W	WbN
Tuesday ...	21	31	56	29.68	29.60	SE	ESE
Wednesday.	22	39	49	29.40	29.40	ESE	NE
Thursday ..	23	42	51	29.50	29.60	NE	NE
Friday	24	38	44	29.60	29.70	NE	NE
Saturday...	25	37	49	29.70	29.90	NNE	NE
Sunday	26	29	52	30.00	29.97	NE	ESE
Monday....	27	35	53	29.68	29.56	SW	NW
Tuesday ...	28	37	53	29.60	29.20	SW	WNW
Wednesday.	29	34	46	29.54	29.73	NW	NW
Thursday ..	30	29	49	29.80	29.72	WbN	W

FOR MAY, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Friday.....	1	43	57	29.52	29.50	W	WbS
Saturday ..	2	42	61	29.50	29.50	WbS	WSW
Sunday....	3	45	62	29.53	29.46	SW	SW
Monday....	4	37	61	29.58	29.80	W	W
Tuesday ...	5	35	59	29.80	29.80	SSE	SW
Wednesday.	6	51	63	29.72	29.80	WbS	SW
Thursday ..	7	41	59	29.80	29.90	S	WbN
Friday	8	41	65	29.94	29.98	WbN	SW
Saturday...	9	45	64	29.98	29.93	WbS	W
Sunday	10	39	68	29.93	29.92	NW	E
Monday ...	11	47	66	29.92	29.88	E	NE
Tuesday ...	12	46	66	29.88	29.81	E	EbS
Wednesday.	13	39	66	29.80	29.80	EbN	SE
Thursday ..	14	39	66	29.80	29.80	SW	W
Friday.....	15	39	65	29.80	29.84	WbN	EbS
Saturday...	16	43	61	29.90	29.90	WbN	NW
Sunday	17	36	65½	29.99	29.97	SW	SE
Monday .. .	18	34	67	29.95	29.85	SE	SE
Tuesday ...	19	37	69	29.80	29.80	E	NE
Wednesday.	20	44	71	29.80	29.82	NE	E
Thursday ..	21	44	62	29.95	29.98	NE	NE
Friday	22	37	63	29.98	29.98	NE	NE
Saturday...	23	42	71	30.00	30.00	NE	SE
Sunday	24	41	67½	30.00	30.03	W	NNW
Monday ...	25	42	59	30.24	30.28	NNE	NE
Tuesday ...	26	39	58	30.32	30.24	NE	NE
Wednesday.	27	42	64	30.21	30.15	NE	NE
Thursday ..	28	42	66	30.11	30.07	NE	EbN
Friday	29	41	67	30.07	30.07	NE	NE
Saturday...	30	43	61	30.07	30.02	NE	NE
Sunday	31	48	67	30.04	29.98	NW	NW

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